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Library of the University of Michigan
The Cook Collection
Miss Jean S. Cook
of Detroit
in memory of her brother
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—— Exhibit of Mr. T. E. Heath's stereoscopic charts of the stars... ix

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his veto whenever they are introduced. I, with diffidence, presume the practical branches of Physic does not mean drugs and medicine, but surgery and operative work. The difference between British Politics and General Politics seems to me to be somewhat obscure, for in the very first volume there is an article which is political and British on "The Impropriety of allowing a Bounty to encourage the exportation of Corn," to which I will allude later.

Medals were awarded to those who read papers. If they were of merit the award of a medal entitled the Member to be enrolled amongst the list of honorary members.

The founders of the Society were the chief Scientific men of Manchester. Among the Honorary Members were Erasmus Darwin, Dr. Franklin, Lavoisier, Dr. Priestley, William Roscoe of Liverpool, the poet and grandfather of Sir Henry Roscoe, the translator of the "Life of Benvenuto Cellini," author of the "Life of Pope Leo the 10th." and also famous as the author of the life of Lorenzo de' Medici, Dorning Rasbottom, a kinsman of my family, Josiah Wedgewood and others, equally distinguished in literature and science.

The Reverend Samuel Hall has a paper in the first volume which would have been anathematised by Mr. Ruskin. The reverend gentleman shews clearly that the taste for nature and beauty and fine arts has no influence favourable or otherwise to morals. I hope that I may not be called a Philistine if I say that I believe the reverend gentleman's judgment is correct.

There is a useful paper by Mr. Thomas Barnes, who says there are some sciences of importance to those who wish to be supreme in manufactures. He also observes that there are many machines in Birmingham which the

Manchester mechanics know nothing at all about, and he hopes shortly to see that the new fire engine (meaning the steam engine) will be used in this district.

There is a foot-note informing the reader that a machine for spinning cotton, probably Crompton's mule, has been working for some time amongst the spinners in Manchester, and there is another being erected for grinding corn and in a state of forwardness near Blackfriars Bridge, London. Although the first great departure in weaving, Kay's fly-shuttle, had been at work nearly 50 years, he proceeds to say that it is not known to a single weaver in the Norwich trade.

He advocates with considerable force the creation of a school in the shape of a museum in which all the various appliances known to man for spinning and weaving and the mechanical arts should be exhibited, with a Professor who knows all these things and is able to describe them to the students, and who should be well versed in mechanical and chemical knowledge. Students should go to it after they had become accomplished in reading, writing, and arithmetic, indeed, he advocated a secondary technical school. The man in charge of it would be, he says, a kind of general Oracle, who might be consulted on mechanical movements which students might find difficult to understand unaided. He wisely concludes, "in a town like this Manchester, the opulence, and even the very existence of which depends on manufactures, and these again upon arts, machinery, and invention, a Public Cabinet, devoted to this purpose would be at once of general ornament and utility."

This remarkable paper was read on January 9th, 1782, and as we know, with the exception of this Society, which has been an Academy of Science, and the Manchester Mechanics Institution, founded 40 years later, the teaching

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CONTENTS.

MEMOIRS.

INAUGURAL ADDRESS. By the President, SIR W. H. BAILEY. ... ... pp. 1—11

I. Note on the Buccal Pits of Peripatus. By C. GORDON HEWITT, B.Sc. *With plate.* ... ... pp. 1—8
(Issued separately, October 21st, 1905).

II. Some Convection Effects in a Heated Tube. By C. H. BURGESS, M.Sc. ... . pp. 1—3
(Issued separately, December 23rd, 1905.)

III. Remarks on the Germinal Layers of Vertebrates and on the Significance of Germinal Layers in general. By J. W. JENKINSON, M.A., D.Sc. *34 Figs.* ... ... pp. 1—89
(Issued separately, March 26th, 1906).

IV. Battack Printing in Java, with Notes on the Malay Kris and the Bornean Sumpitan and Upas Poison. By JOHN ALLAN ... ... pp. 1—13
(Issued separately, April 7th, 1906).

V. Report on the Recent Foraminifera from the Coast of the Island of Delos (Grecian Archipelago). Part III. By HENRY SIDEBOTTOM. *Plates I—II.* ... ... pp. 1—18
(Issued separately, April 19th, 1906).

VI. The Cytological Aspect of Parthenogenesis in Insects. By C. GORDON HEWITT, B.Sc. *Plates I—II.*... ... pp. 1—38
(Issued separately, May 21st, 1906).

VII. Total Solar Eclipses. (The Wilde Lecture). By Professor H. H. TURNER, D.Sc., F.R.S. ... ... pp. 1—32
(Issued separately, May 26th, 1906).

VIII. On the Range of Progressive Waves of Finite Amplitude in Deep Water. By R. F. GWYTHER, M.A. ... ... pp. 1—28
(Issued separately, May 31st, 1906).

IX. Observations on a Captive Mole (*Talpa europæa*). By LIONEL E. ADAMS, B.A. ... ... pp. 1—7
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his veto whenever they are introduced. I, with diffidence, presume the practical branches of Physic does not mean drugs and medicine, but surgery and operative work. The difference between British Politics and General Politics seems to me to be somewhat obscure, for in the very first volume there is an article which is political and British on "The Impropriety of allowing a Bounty to encourage the exportation of Corn," to which I will allude later.

Medals were awarded to those who read papers. If they were of merit the award of a medal entitled the Member to be enrolled amongst the list of honorary members.

The founders of the Society were the chief Scientific men of Manchester. Among the Honorary Members were Erasmus Darwin, Dr. Franklin, Lavoisier, Dr. Priestley, William Roscoe of Liverpool, the poet and grandfather of Sir Henry Roscoe, the translator of the "Life of Benvenuto Cellini," author of the "Life of Pope Leo the 10th." and also famous as the author of the life of Lorenzo de' Medici, Dorning Rasbottom, a kinsman of my family, Josiah Wedgewood and others, equally distinguished in literature and science.

The Reverend Samuel Hall has a paper in the first volume which would have been anathematised by Mr. Ruskin. The reverend gentleman shews clearly that the taste for nature and beauty and fine arts has no influence favourable or otherwise to morals. I hope that I may not be called a Philistine if I say that I believe the reverend gentleman's judgment is correct.

There is a useful paper by Mr. Thomas Barnes, who says there are some sciences of importance to those who wish to be supreme in manufactures. He also observes that there are many machines in Birmingham which the

Manchester mechanics know nothing at all about, and he hopes shortly to see that the new fire engine (meaning the steam engine) will be used in this district.

There is a foot-note informing the reader that a machine for spinning cotton, probably Crompton's mule, has been working for some time amongst the spinners in Manchester, and there is another being erected for grinding corn and in a state of forwardness near Blackfriars Bridge, London. Although the first great departure in weaving, Kay's fly-shuttle, had been at work nearly 50 years, he proceeds to say that it is not known to a single weaver in the Norwich trade.

He advocates with considerable force the creation of a school in the shape of a museum in which all the various appliances known to man for spinning and weaving and the mechanical arts should be exhibited, with a Professor who knows all these things and is able to describe them to the students, and who should be well versed in mechanical and chemical knowledge. Students should go to it after they had become accomplished in reading, writing, and arithmetic, indeed, he advocated a secondary technical school. The man in charge of it would be, he says, a kind of general Oracle, who might be consulted on mechanical movements which students might find difficult to understand unaided. He wisely concludes, "in a town like this Manchester, the opulence, and even the very existence of which depends on manufactures, and these again upon arts, machinery, and invention, a Public Cabinet, devoted to this purpose would be at once of general ornament and utility."

This remarkable paper was read on January 9th, 1782, and as we know, with the exception of this Society, which has been an Academy of Science, and the Manchester Mechanics Institution, founded 40 years later, the teaching

of the first principles of science and the industrial arts in this district was unknown and neglected.

There are in this first volume two papers by J. Wimpey, "On Economical Registers" and "On the impropriety of allowing a bounty to encourage the exportation of Corn." In the paper on economical registers which would be called statistics now, (indeed only about 1833, the year in which the first Statistical Society in England was founded in Manchester, had the word become common), he asserts that we took no census of the population at this period, indeed, it was considered wicked to take a census, and only in 1801 did we count our population. We did not know, said Mr. Wimpey, the amount of cash circulation in the kingdom, or the state of the population or its health.

He shows the absurdity of paying four shillings the quarter on all exportations of corn, especially when our crops had been bad and corn was scarce for our people; he says, if we had an economical register we might regulate the export of corn according to our necessities, for we then exported more than we consumed, and observes: "So keen is pursuit of private emolument, and so ignorant and remiss is the Government, that they have frequently given a bonus of fifteen per cent. to export corn, when all they had in stock was very far short of being sufficient to support their own people until the next harvest." With indignation he says, "The Dutch by buying this corn and sending it back had made millions of money out of foolish England." He further proceeds to say, "that our legislators have been very fruitful in the invention of penal laws; but in the measures of prevention, which are infinitely more salutary, they are either very inattentive or very barren."

He incidentally alludes to the unemployed and ne'er-

do-wells. He would give every man a medal with his name on who was engaged in labour, and unless he could produce it he would punish him; but "liberty is in such high esteem, fears are awakened, suspicions alarmed, jealousies excited, lest any encroachment should be made on the liberty of the subject, under the specious but deceitful appearance of public good." Then he returns to the corn bounties again, and insists that we ought to have knowledge of our production, for "it is a question of the greatest importance in this country, ignorance of which has cost millions of pounds." He very wisely continues, "No Government can be just whose support and defence does not extend to the equal, and indiscriminate benefit of the whole people" He denounces the conflict between the landed interests and the commercial classes, and tersely says, "There is a bias, for where shall we find those who have no interests in the laws they enact." He does not believe what is said in Parliament, that a bounty on the exportation of corn has benefited the landowner and the farmer, and added to the riches of the nation. He points out that this export bounty has been paid to the corn growers for 95 years, and advocates that in times of great abundance the corn surplus should be stored in public granaries, and in times of scarcity it should be sold, and the extra price the growers obtained in bad years would be better for the people and the nation, than sending it out of the country and making it so dear that people scarcely get any corn at all. It appears that in 1795 the quartern loaf was one shilling, and in 1800, one shilling and fivepence. In those days working people never saw wheaten bread. This paper is well worthy the study of those who are interested in fiscal questions.

Mr. Wimpey is a thorough protectionist in other

respects. He agrees that it is proper to prohibit the import of French silks into this country, and those from our possessions in India were also prohibited under very severe penalties.

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In our "Memoirs" will be found records of the work of our members in their investigations of the economics of industry, the scientific uses of energy, the properties of steam, the very elementary beginnings of electricity (that for years was looked upon as a toy), and also those in connection with the early manufactures, dyeing, bleaching, &c., all of which have a fascinating interest to those who are practically engaged in our industrial work, or to the historical student.

The chief tools of the workshops of the world, not only of those where steam engines, locomotives, and steamships are built, but also of the textile factories of the world, were invented in Manchester or within thirty miles of it; and our records contain the names of many of these inventors who were members, for the men of this County were the first to use steam power for spinning and weaving, and for punching, cutting, shaping metal; they were to the front in changing the old handicraft machines to suit the new steam power. Prominent among them was that genius, Richard Roberts, who was one of the founders of the Manchester Mechanics Institution and always in the front rank in advocating technical education. He was the most prolific of them all—his chief inventions being the slide lathe, planing machine, and self-acting

mule for spinning cotton, and a number of others for steamships, screw propellers, life-boats and armour-clads, &c. Then we had Nasmyth, the inventor of the steam-hammer, a man who loved science more than money, for in his middle age he retired to his garden in the South of England, and studied the lamps of heaven with a great telescope he designed and made at Patricroft. Mention should also be made of Sir William Fairbairn and Sir Joseph Whitworth. Dr. Priestley, who discovered oxygen, occasionally visited us. Count Rumford, who tried to discover the mechanical equivalent of heat, was amongst our honorary members, as was also Dr. Darwin's grandfather, the celebrated Erasmus, author of the Botanic Garden, and the poet and prophet of the future of the steam engine, who with exultation wrote :—

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absolutely unaided by Government or other outside assistance. Is it overstating the case to assert that the Victoria University and the Manchester Technical School are the direct results of the advocacy of the members of this old Society?

The site of the Old Infirmary has been called Manchester's great opportunity. It is known to some of us that members of the Manchester City Council are considering how this fine site may be used.

We have confidence that an Institution will be erected worthy of the birthplace of Free Municipal Libraries, and that literature, art, and science will be recognised in a good, broad, generous and artistic manner.

Some of us are not without hope that space will be found in this Institution for a Hall of Fame, many of which are on the Continent, and one especially, I have in my mind, at Munich, in which may be placed portraits and sculptures that will cause us to hold in perpetual remembrance the names and services of the illustrious benefactors, inventors, and pious founders of this great County of Lancaster, whose work, a perpetual endowment, is in the class books of the schools of the world, where chemistry, electricity, and engineering science are taught.

Such a shrine would exalt the self-respect of every thoughtful citizen, and be an inspiration to the imagination of every student, for, in the topographical distribution of men of genius, this county has a rich record of inventors, dramatists, statesmen, discoverers, poets and painters, and moulders of the mind of man

> A noble company, the flower of men,
> To serve as model for a mighty world.

Manchester Memoirs, Vol. l. (1905), *No.* 1.

I. Note on the Buccal Pits of Peripatus.

By C. GORDON HEWITT, B.Sc.,

Demonstrator of Zoology in the University of Manchester.

(*Communicated by Professor S. J. Hickson, F.R.S.*).

Received and Read, October 3rd, 1905.

The earliest detailed account of these structures* is that given in a memoir on *Peripatus capensis* by Balfour (1), who, after describing the characters of the outer and inner pairs of jaws, adds (p. 222)—"A more important difference between the two blades than in the character of the cutting edge just spoken of, is to be found in their relation to the muscles which move them. The anterior parts of both blades are placed on two epithelial ridges, which are moved by muscles common to both blades (*pl.* xvi., *fig.* 11). Posteriorly, however, the behaviour of the two blades is very different. The epithelial ridge bearing the outer blade is continued back for a short distance behind the blade, but the cuticle covering it becomes very thin, and it forms a simple epithelial ridge placed parallel to the inner blade. The cuticle covering the epithelial ridge of the inner blade is, on the contrary, prolonged behind the blade itself as a thick rod, which, penetrating backwards along a deep pocket of the buccal epithelium, behind the main part of the buccal cavity for the whole length of the pharynx, forms a very powerful

* In Moseley's account (2) of this species the following words may refer to these structures:—"From the posterior part of the lateral surfaces of the pharynx, a pair of small muscles which probably are protractors of the pharynx, and serve to push forward the jaws."

lever, on which a great part of the muscles connected with the jaws find their insertion. The relations of the epithelial pocket bearing this lever are somewhat peculiar.

"The part of the epithelial ridge bearing the proximal part of this lever is bounded on both its outer and inner aspect by a deep groove. The wall of the outer groove is formed by the epithelial ridge of the outer blade, and that of the inner by a special epithelial ridge at the side of the tongue. Close to the hinder border of the buccal cavity (as shown in *pl.* xvi., *fig.* 12, on the right hand side) the outer walls of these two grooves meet over the lever, so as to completely enclose it in an epithelial tube, and almost immediately behind this point the epithelial tube is detached from the oral epithelium, and appears in section as a tube with a chitinous rod in its interior, lying freely in the body cavity (shown in *pl.* xvi., *figs.* 13—16 *le*). This apparent tube is the section of the deep pit already spoken of. It may be traced back even beyond the end of the pharynx, and serves along its whole length for the attachment of muscles."

I have not had the opportunity of examining sections of these jaw-levers in *P. capensis*, but have done so in series of sections of *P. balfouri*, *P. novae zealandiae* and *Ooperipatus oviparus* (Dendy) and in every case the chitinous rod was hollow and not solid as described and figured by Balfour in the case of *P. capensis*. The hollow nature of these jaw levers has already been described by Prof. Dendy in his memoir on the oviparous species of Peripatus (3). He assigns a respiratory function to these hollow jaw-levers, which form the buccal pits, as will be seen from the following description of them, which he gives. Referring to *O. oviparus*, he says (p. 371): "In this species (and probably in the others) there is a tracheal pit immediately in front of the mouth, and a pair of very

large ones opening into the buccal cavity just behind and close to the base of the inner jaw on each side, and running backwards for some distance, at first just outside the lateral nerve cords (*fig.* 5, *B. tr.*) and then above and just inside the salivary glands. These buccal tracheal pits have a thick chitinous lining, and may be traced back in a series of transverse sections very nearly to the level of the second pair of legs. They give off along their course and from their extremities an immense number of very fine tracheal tubes. When the jaws are removed, these enormously elongated tracheal pits may be pulled out in connection with them, and the chitinous lining of the pit appears to pass over into the chitinous covering of the smallest accessory tooth of the inner jaw."

On comparing these two accounts it is quite evident that they are descriptions of similar structures; but there is a striking difference in the characters and functions ascribed to them. In Balfour's account there is a solid chitinous jaw-lever in an epithelial pocket, to which are attached "a great part of the muscles connected with the jaws;" whereas Dendy describes them as buccal tracheal pits having a chitinous lining and giving off "along their course and from their extremities an immense number of very fine tracheal tubes." On the other hand their figures are very similar.

In working over series of sections of *P. balfouri* and *P. novae zealandiae* I suspected that Prof. Dendy had mistaken the striated muscle fibres which move the jaw-levers for tracheal tubes and "enormously elongated tracheal pits." I called his attention to this, and he kindly sent me his sections of *Ooperipatus oviparus* from which his description and figures had been made. An examination of these convinced me that this was the case, and that the supposed tracheal structures were muscles in *O. oviparus*

as Balfour had described them in *P. capensis*, and as I had found them to be in *P. balfouri* and *P. novae zealandiae.*

The muscle fibres which work the jaw-levers are transversely striated, being the only transversely striated muscle fibres which occur in Peripatus. The fine tracheæ are also striated, but on comparison it is found that the striations of the tracheæ differ considerably from those of the muscle fibres. As Moseley showed (2) in the first account which was given of the tracheæ of Peripatus, the tracheæ are provided with a spiral thread, the individual coils of which are fairly wide apart. Their walls are extremely thin, and hard to detect, except with a high power. The muscle fibres have a more dense appearance than the tracheæ; this is due to the fact that the striations of the muscle fibres are very fine and close together, and stain very readily.

I was unable to find in any of the sections examined any apertures in the epithelial pockets into which the chitinous jaw-levers fit, which might serve as means of communication between the buccal pits and any tracheal structures, supposing the latter existed in this region. Certainly no tracheal pits were given off from these epithelial pockets.

In the species examined the characters of these structures are very similar, the only difference being in the form of the jaw-lever, which in *P. balfouri* differs slightly from those of *P. novae zealandiae* and *O. oviparus.* As Balfour shows, the cuticle covering the epithelial ridge of the inner blade is prolonged backwards as a thick, flat, chitinous rod. In the anterior region of this rod the outer walls of the two grooves described by him meet at the hinder end of the buccal cavity. These grooves are lined by a thin cuticle, and when the fusion of the outer walls of the grooves takes place, the cuticle also

fuses, so that a hollow, chitinous rod communicating with the buccal cavity is formed. In each of these species the chitinous rod is flat and fluted, so that in section it has a wavy appearance (see *Figs.* 1, 2, *le.*). In *P. novae zealandiae* and *O. oviparus* the inner wall of the rod is thin, while the portion adjacent to the attachments of the muscles is much thicker.*

The specimens of *P. balfouri* examined had these two walls of equal thickness, as shown in the accompanying figures. These walls are in all cases thick in the middle, and gradually become thinner, as seen in transverse section, towards their edges, where they join to form the flat, hollow, chitinous jaw-lever. In some sections the rod has a solid appearance, the two sides having collapsed. The character of the epithelial pocket, into which the jaw-lever fits, is the same in each species. It is made up of a single layer of cubical cells containing large nuclei (*Fig.* 2), and is simply a deep pocket of the epithelium lining the buccal cavity. In no section is the continuity of this cellular layer broken, either at the edges or at the sides of the chitinous rod. The ends of these epithelial pockets are quite blind, and consist of a solid mass of epithelial cells. The chitinous jaw-lever is also imperforate (except in certain sections where the perforation is clearly an artifact).

To sum up, the continuity of the cellular layer of the epithelial pocket, and the imperforate nature of the hollow, chitinous jaw lever, render the presence of tracheal struc-

* The sections of *P. novae zealandiae* and *P. balfouri* were cut 8*u* thick, and stained with hæmalin (4). This reagent stains the chitin a deep, purplish black, so that the thin inner wall is rendered perfectly visible. Unless this thin inner wall is stained, it may be mistaken for an optical effect caused by the balsam and the inner wall of the epithelial pocket, consequently the jaw-lever would appear to be a solid chitinous rod instead of a hollow chitinous rod.

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large ones opening into the buccal cavity just behind and close to the base of the inner jaw on each side, and running backwards for some distance, at first just outside the lateral nerve cords (*fig.* 5, *B. tr.*) and then above and just inside the salivary glands. These buccal tracheal pits have a thick chitinous lining, and may be traced back in a series of transverse sections very nearly to the level of the second pair of legs. They give off along their course and from their extremities an immense number of very fine tracheal tubes. When the jaws are removed, these enormously elongated tracheal pits may be pulled out in connection with them, and the chitinous lining of the pit appears to pass over into the chitinous covering of the smallest accessory tooth of the inner jaw."

On comparing these two accounts it is quite evident that they are descriptions of similar structures; but there is a striking difference in the characters and functions ascribed to them. In Balfour's account there is a solid chitinous jaw-lever in an epithelial pocket, to which are attached "a great part of the muscles connected with the jaws;" whereas Dendy describes them as buccal tracheal pits having a chitinous lining and giving off "along their course and from their extremities an immense number of very fine tracheal tubes." On the other hand their figures are very similar.

In working over series of sections of *P. balfouri* and *P. novae zealandiae* I suspected that Prof. Dendy had mistaken the striated muscle fibres which move the jaw-levers for tracheal tubes and "enormously elongated tracheal pits." I called his attention to this, and he kindly sent me his sections of *Ooperipatus oviparus* from which his description and figures had been made. An examination of these convinced me that this was the case, and that the supposed tracheal structures were muscles in *O. oviparus*

as Balfour had described them in *P. capensis*, and as I had found them to be in *P. balfouri* and *P. novae zealandiae.*

The muscle fibres which work the jaw-levers are transversely striated, being the only transversely striated muscle fibres which occur in Peripatus. The fine tracheæ are also striated, but on comparison it is found that the striations of the tracheæ differ considerably from those of the muscle fibres. As Moseley showed (2) in the first account which was given of the tracheæ of Peripatus, the tracheæ are provided with a spiral thread, the individual coils of which are fairly wide apart. Their walls are extremely thin, and hard to detect, except with a high power. The muscle fibres have a more dense appearance than the tracheæ; this is due to the fact that the striations of the muscle fibres are very fine and close together, and stain very readily.

I was unable to find in any of the sections examined any apertures in the epithelial pockets into which the chitinous jaw-levers fit, which might serve as means of communication between the buccal pits and any tracheal structures, supposing the latter existed in this region. Certainly no tracheal pits were given off from these epithelial pockets.

In the species examined the characters of these structures are very similar, the only difference being in the form of the jaw-lever, which in *P. balfouri* differs slightly from those of *P. novae zealandiae* and *O. oviparus.* As Balfour shows, the cuticle covering the epithelial ridge of the inner blade is prolonged backwards as a thick, flat, chitinous rod. In the anterior region of this rod the outer walls of the two grooves described by him meet at the hinder end of the buccal cavity. These grooves are lined by a thin cuticle, and when the fusion of the outer walls of the grooves takes place, the cuticle also

fuses, so that a hollow, chitinous rod communicating with the buccal cavity is formed. In each of these species the chitinous rod is flat and fluted, so that in section it has a wavy appearance (see *Figs.* 1, 2, *le.*). In *P. novae zealandiae* and *O. oviparus* the inner wall of the rod is thin, while the portion adjacent to the attachments of the muscles is much thicker.*

The specimens of *P. balfouri* examined had these two walls of equal thickness, as shown in the accompanying figures. These walls are in all cases thick in the middle, and gradually become thinner, as seen in transverse section, towards their edges, where they join to form the flat, hollow, chitinous jaw-lever. In some sections the rod has a solid appearance, the two sides having collapsed. The character of the epithelial pocket, into which the jaw-lever fits, is the same in each species. It is made up of a single layer of cubical cells containing large nuclei (*fig.* 2), and is simply a deep pocket of the epithelium lining the buccal cavity. In no section is the continuity of this cellular layer broken, either at the edges or at the sides of the chitinous rod. The ends of these epithelial pockets are quite blind, and consist of a solid mass of epithelial cells. The chitinous jaw-lever is also imperforate (except in certain sections where the perforation is clearly an artifact).

To sum up, the continuity of the cellular layer of the epithelial pocket, and the imperforate nature of the hollow, chitinous jaw-lever, render the presence of tracheal struc-

* The sections of *P. novae zealandiae* and *P. balfouri* were cut 8*u* thick, and stained with brazilin (4). This reagent stains the chitin a deep, purplish black, so that the thin inner wall is rendered perfectly visible. Unless this thin inner wall is stained, it may be mistaken for an optical effect caused by the balsam and the inner wall of the epithelial pocket, consequently the jaw-lever would appear to be a solid chitinous rod instead of a hollow chitinous rod.

tures, such as tracheal pits and tracheæ in connection with them, highly improbable.

The muscles which work the jaw-levers are attached to the dorso-lateral walls of the epithelial pocket, as shown in the figures, chiefly about the anterior and median portions of the rod, the larger muscles disappearing towards the posterior extremity. They consist of two series, a median series (*Fig. 2, lm*) running in a longitudinal direction enclosed by two transverse bands of muscle (*tm*) which are attached to the greater part of the dorso-lateral side of the epithelial pocket, that is, to the whole surface of this side except in the middle line (as shown in *Fig. 2*) and extend thence dorso-laterally. The jaw-levers and their muscles disappear opposite the beginning of the second pair of legs.

Prof. Dendy has pointed out to me that Purcell (5) has shown that in the Attidæ, a group of spiders, "by far the greater part of the tracheal system is nothing else but a pair of modified ectodermal tendons." If ectodermal tendons can be modified in this way,* the question naturally arises, can these hollow chitinous jaw-levers of Peripatus be compared with ectodermal tendons which in the Attidæ have become modified, according to Purcell, to form tracheal structures. I do not think that the hollow chitinous jaw-levers of Peripatus can be regarded as structures of a respiratory nature. They are simply skeletal structures, flattened for the better attachment of the muscles which control them, and hollow like many structures of a similar nature found in other Arthropods.

* Korschelt & Heider (6) are inclined to trace back the origin of the tracheæ of the Arachnida to lungs. Simmons (7) has found that the tracheæ develop from foldings in the posterior surface of the third abdominal appendage, which in their early stages are similar to those on the second appendage, which give rise to the lung-books. He considers that the tracheæ are developed from a lung-book condition.

SUMMARY.—It has been shown that the so-called buccal tracheal pits of *Ooperipatus oviparus*, described by Prof. Dendy, are really hollow chitinous jaw-levers; the striated muscle fibres which make up the muscles controlling them, having been mistaken for tracheal tubes.

In conclusion, I wish to thank Prof. Dendy for his kind assistance, and for supplying me with specimens of *P. balfouri*.

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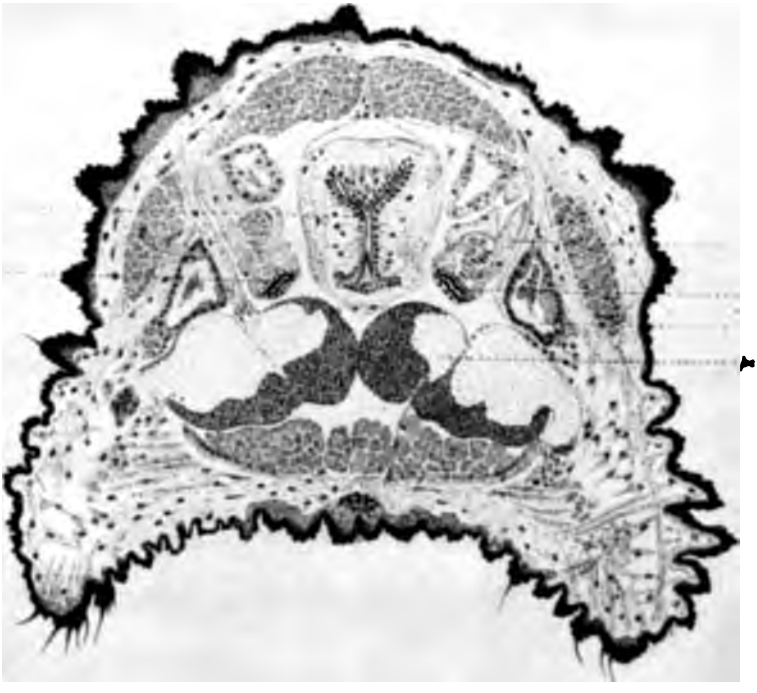
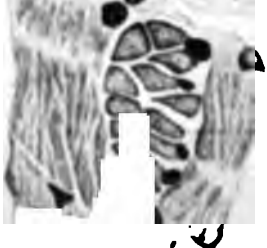
EXPLANATION OF PLATE.

Fig. 1. Transverse section of *P. balfouri*, through the middle of the first pair of legs.

Fig. 2. Enlarged view of transverse section of the jaw-lever of the right side and its attachments, in the region of the first pair of legs.

bp. Buccal pit. *ep.* Epithelial pocket. *le.* Chitinous jaw-lever. *lm.* Longitudinal muscles of jaw-lever. *pc.* Posterior lobe of brain. *ph.* Pharynx. *sld.* Reservoir of slime gland. *tm.* transverse muscles of jaw-levers.





II. Some Convection Effects in a Heated Tube.

By C. H. BURGESS, M.Sc.

Received and Read, November 14th, 1905.

W. C. D. Whetham, in his "Experiments on Ionic Velocities" [*Phil. Trans.* A, 1893, 337], half filled a V-tube, of special construction, with a red alcoholic solution of cobalt nitrate. A layer of a specifically lighter blue solution of cobalt chloride in alcohol was placed above it.

On passing an electric current through the apparatus, a number of purple lines were formed at the junction of the two solutions and the lines travelled both with and against the current. Whetham could explain this behaviour only by the presence of coloured complex anions in the solution.

During some similar experiments I filled the upper portion of a U-tube with hydrochloric acid and the lower portion with a specifically heavier solution of cobalt chloride in the same acid. After standing overnight the blue liquid had diffused up into the colourless, giving a gradually shaded band. On passing a current of half an ampère through the tube by means of carbon electrodes, the band was resolved into a series of layers of gradated shades, with a sharp line of demarcation between each.

To determine whether this was due in any way to complex ions, the blue cobalt chloride solution was replaced by hydrochloric acid, to which a red dye and a little glycerine (to render it heavier had been added.

After diffusion had taken place the step-like layers were formed by the current.

December 23rd, 1905.

This is seen in *Fig.* 1.

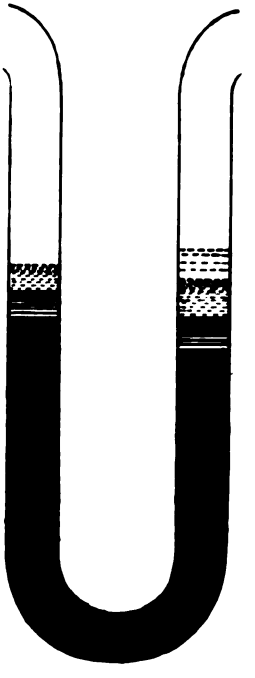


Fig. 1.

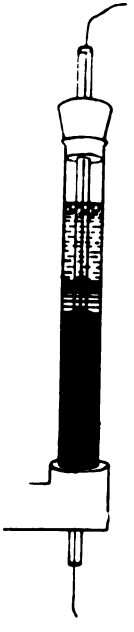


Fig. 2.

This indicated that the effect was due to heating, the centre of the tube being hotter than the walls, which were cooled by the air.

With a strong current it was possible to see the hot coloured liquid rise in the centre, and spread out into a mushroom shape. The cold liquid from the walls falling underneath broke off the head of the column, giving a new layer.

Water coloured by a dye was then placed at the bottom of a vertical tube, with plain water above.

A platinum wire enclosed in a thin glass tube was inserted down the centre.

The wire was heated electrically, and after a time a number of striæ made their appearance.

They gradually broadened, and formed a series of layers which were sharp at the top, and then faded away until the next layer was reached (*Fig.* 2).

The effect is not quite so well marked as when the liquid is heated directly, and so more uniformly by the current.

The sharpness and permanency of these boundaries seems to indicate that there are vortex movements due to convection currents between them.

The pressure is greater at the walls, and the liquid rises in the centre. Its place is taken by the cold liquid from the outside, which rises in its turn, thus causing a circular motion from the wire to the walls, between the layers.

The breadth of the bands appears to depend directly on the difference of temperature of the inside and outside of the liquid.

Increasing the current sometimes even causes the lines to become doubled, the original ones gradually being effaced.

If the tube is heated simultaneously from the inside and outside by coiling another wire round the tube, the layers do not appear; there is, however, a tendency to set up large convection currents, which rapidly mix the liquids.

As this is passing through the press, I notice an abstract in *Central Blatt, 1905*, B II, No. 23 of a paper by C. Christiansen, (*Overs. o.d. Kgl. Dansk. Vidensk. Selskab Forh. 1905*, 307-15,) who has obtained similar results, and formed much the same conclusions as those given above.

Manchester Memoirs, Vol. l. (1906), *No.* 3.

III. Remarks on the Germinal Layers of Vertebrates and on the Significance of Germinal Layers in general.

By J. W. JENKINSON, M.A., D.Sc.

Exeter College, Oxford.

(Communicated by Dr. F. W. Gamble.)

Received January 12th, 1906. Read January 15th, 1906.

INTRODUCTION.

That theory plays a part of predominant importance in the acquisition of new facts is a matter of common knowledge, and embryology has afforded no exception to this rule. The cell theory, the recapitulation theory, and theories of germinal layers have here not only supplied a stimulus which has produced but also provided conceptions which have dominated and guided a long series of developmental investigations. In the light of these theories the embryologist has set himself to complete the task already taken in hand by the comparative anatomist, the search for homologies, believing that he has had in the homology of the germinal layers an absolute and infallible criterion of the homogeny, or community of descent, of the organs of the adult.

Now while it is true that the morphologists of the older school and their pupils, to say nothing of the more

[1] I have to thank Dr. Haldane, Dr. Ritchie, Dr. Bourne, and Mr. Assheton for their kindness in reading through the manuscript of this paper; to Mr. Assheton I am particularly indebted for much friendly and valuable criticism.

March 26th, 1906.

gists, I mean the relation of the Amniote blastopore to that of the Anamnia.[2]

Fatal, however, as I believe the evidence of Vertebrate development to be to the common views, the testimony available from other sources must also be taken into account; I have, therefore, supplemented my critique by a consideration of the embryogeny—in particular of the cell lineages—of various Invertebrates, and of the bearing of all the facts of budding, regeneration, pathology, and experimental embryology on the problem.

The conclusion to which I have come I have already hinted at; unacceptable as it may prove to many, it is the only conclusion which, as far as I can see, is compatible with the facts.[3]

Part I.—THE GERMINAL LAYERS OF THE VERTEBRATA.

ANAMNIA.

As a type of the formation of the germinal layers in the Anamnia, I will take the form which has been most extensively studied, and with which I am myself most intimately acquainted, the common English Frog (*Rana temporaria*).

Let me say at the outset that I define these layers—ectoderm, the endoderm, and the mesoderm—solely reference to their destiny;[4] by the "ectoderm" I

A complete literature of the germ-layers of the vertebrates will be in Hertwig's "Handbuch" Cap. 3, (Gustav Fischer, Jena, 1903); the reader is referred for particulars of the memoirs quoted in the paper.

nce this paper was written I have found that F. Braem, in a paper "Was ist ein Keimblatt?" and published nearly ten years ago (*Biol. Centr.*, vol. 15, 1895, p. 427—443, 466—476, 491—506), came to s practically identical with my own. Braem's views are founded the developmental phenomena of Bryozoa and Ascidia.

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humble writers of text-books, continue to adhere to the authority of long-established dogma, the student who has attentively studied the embryological work of recent years can hardly have failed to notice a growing feeling of dissatisfaction and discontent with these older generalizations. The adequacy of the cell-theory to give an explanation of ontogenetic processes has been openly called in question in more than one quarter; grave doubts have been expressed of the validity of the fundamental biogenetic law, while the difficulties which every conscientious observer experiences in trying to patch the old theoretical garments of germ-layer hypothesis with the new cloth of descriptive and experimental fact are patent on every side. The time would then seem to be ripe for a renewed, and a critical, examination of the principles involved in those theories which have attached a morphological significance to the primary cell layers of the embryo, and all the more so in a country in which no word has been spoken on the subject since the publication of Francis M. Balfour's *Comparative Embryology* twenty years ago. The views which will be expressed in the sequel were forced upon me in the first instance by a study of the formation of the layers in the Vertebrata; and it is, therefore, on a consideration of these processes as they occur in this group that I shall primarily base my argument. Very fortunately, the recent publication by Oscar Hertwig in his *Handbuch der Entwickelungslehre der Wirbeltiere* of a clear and comprehensive review of the facts has completely absolved me from the necessity of giving more than the briefest account of them, emphasizing only such points as are needful for my purpose; though I have taken the opportunity of discussing a problem which has been the stumbling-block of more than a generation of embryolo-

gists, I mean the relation of the Amniote blastopore to that of the Anamnia.[2]

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[4] Braem's phrase deserves quoting. "Keimblatter," he says, "sind Organanlagen." (p. 431.)

understand that layer or set of cells which will give rise to the epidermis and its derivatives, such as hair, skin-glands, feathers, to the nervous system and organs of special sense, and to the stomodaeum and proctodaeum ; similarly by the " endoderm " I mean the cells which will provide the lining epithelium for the alimentary tract and its outgrowths, the liver, lungs, and so forth ; and by the " mesoderm " the source of the musculature, skeleton, and connective tissue, the blood and blood vessels and the urino-genital system.

THE FROG.

In the Frog the process of germ-layer formation begins when segmentation is completed. The fully segmented ovum consists of an animal hemisphere (rather more than a hemisphere) of small pigmented cells, containing but little yolk, and a vegetative hemisphere of bulky white cells full of large yolk-granules. From the centre of the animal hemisphere (or animal pole) a straight line can be drawn passing through the centre of the egg and the centre of the vegetative hemisphere (the vegetative pole) ; this line is the egg-axis and about it the egg is radially symmetrical. In the normal position of the egg the axis is vertical. Whether, as Schulze has maintained, segmentation has already conferred a bilateral symmetry upon the egg is a point which for our present purpose is immaterial. At the equator of the egg are cells which are in every respect intermediate between the small animal and the large vegetative cells. In its interior is a spacious segmentation cavity, hemispherical in shape, placed axially but excentrically and nearer the animal than the vegetative pole. Its roof is formed of about four layers of small cells, the outermost of which is arranged very distinctly as a cubical or shortly columnar epithelium,

its floor of about twenty layers of large polyhedral yolk-cells.

The first sign of the formation of the germ-layers is given by the appearance of the structure known as the dorsal lip of the blastopore (*Fig.* 1, A). This is a short, deeply pigmented groove, placed parallel to the equator, and a little below it (about 25°) at one point in the boundary between the pigmented and the unpigmented portions of the egg. With the appearance of the dorsal lip the bilaterality of the egg becomes fully established, if it was not already in existence; the plane which includes the egg-axis and the dorsal lip is the sagittal plane of the future embryo.

The changes that now take place, as seen from the vegetative pole, are as follows (*Fig.* 1, B—D):—The groove begins to travel downwards over the surface of the egg towards the vegetative pole, the area over which it passes becoming covered by cells which are as deeply pigmented as those of the animal hemisphere. At the same time the groove elongates, becoming crescentic; in other words, not merely one limited region of the boundary between pigmented and yolk-cells, namely, the dorsal lip, is involved in the process of overgrowth, but the regions lying to the right and left of this, that is to say the lateral lips of the blastopore, as well. As the dorsal lip (the middle region of the groove) continues on its course towards the vegetative pole, and as continually fresh regions are drawn into the process at the sides, the blastoporic lip becomes first semi-circular, and then three parts of a circle, until, finally, when that region, the ventral lip, which is diametrically opposite to the dorsal lip also begins to grow down, it attains the form of a circle enclosing the still uncovered portion of the vegetative hemisphere, the yolk-plug. The dorsal lip has now moved down to, or a little beyond, the vegetative pole.

The movement of the dorsal lip just described is due to its growth over the surface of the egg, the axis of

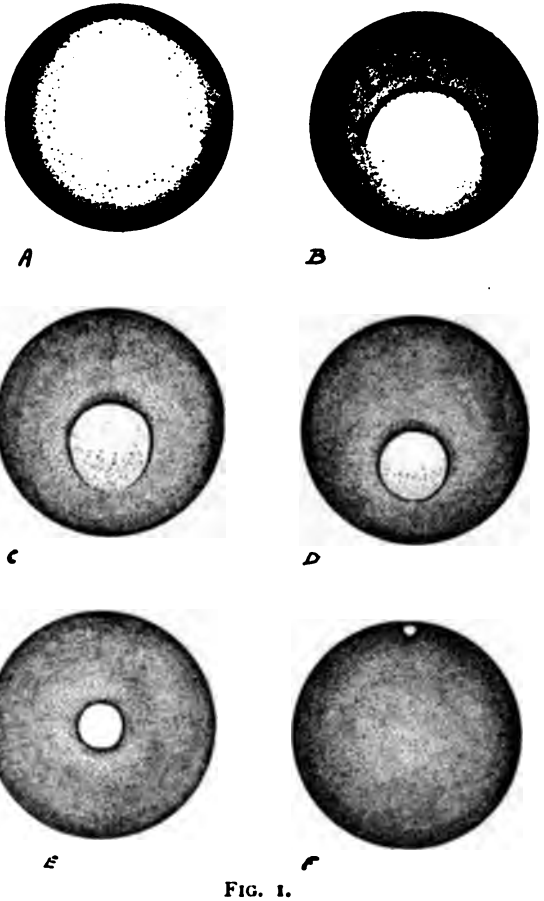

FIG. 1.

A—D. Views of the vegetative hemisphere of the Frog's egg during the overgrowth of the blastoporic lip.

E, F. Views of the lower surface during rotation of the whole egg.

In all the figures the dorsal lip is on the upper side. In *D* the ventral lip is just formed.

(Original.)

which has up to the present retained its original vertical position. At this moment, however, the whole egg begins

to rotate about a horizontal axis in a direction which is the opposite of that in which the dorsal lip had moved (*Fig.* 1, E, F); and this rotation continues—the circle of the blastopore becoming smaller all the time—until the dorsal lip has returned, rather beyond the point from which it started, to the (new) equator of the egg. The angle subtended by the area of the yolk surface which it traverses—both before and during the rotation—is about 75°, as estimated by Kopsch, and the angle through which the whole egg rotates about 100. It follows that the present vertical axis of the egg, which will be the dorso-ventral axis of the embryo, makes the same angle of 100° with the original egg-axis, that the animal pole is situated below what will be the anterior end of the embryo, and that the antero-ventral half of the embryo is developed over the animal, the postero-dorsal half over the vegetative hemisphere. (*Fig.* 2, F).

It has been suggested from time to time that the first movement of the blastopore, as well as the second, is due to a rotation of the egg as a whole, the overgrowth of the pigmented over the unpigmented area being only apparent, and the darkening of the vegetative hemisphere due to the formation of pigment in the superficial yolk-cells. Against this view it must be urged (1) that Kopsch has seen the yolk-cells streaming underneath the overgrowing lip; (2) that if a small exovate is produced by lightly puncturing the egg at the animal pole, this is afterwards found in front of the medullary groove;[1] (3) that when the blastopore is experimentally prevented from closing the anterior end of the embryo is seen to develope at the animal pole; and (4) that there is a cause, namely, shifting of the yolk and consequent displacement of the

[1] Mr. Assheton has pointed out to me that this evidence is not conclusive, as the exovates may become detached and shift their position.

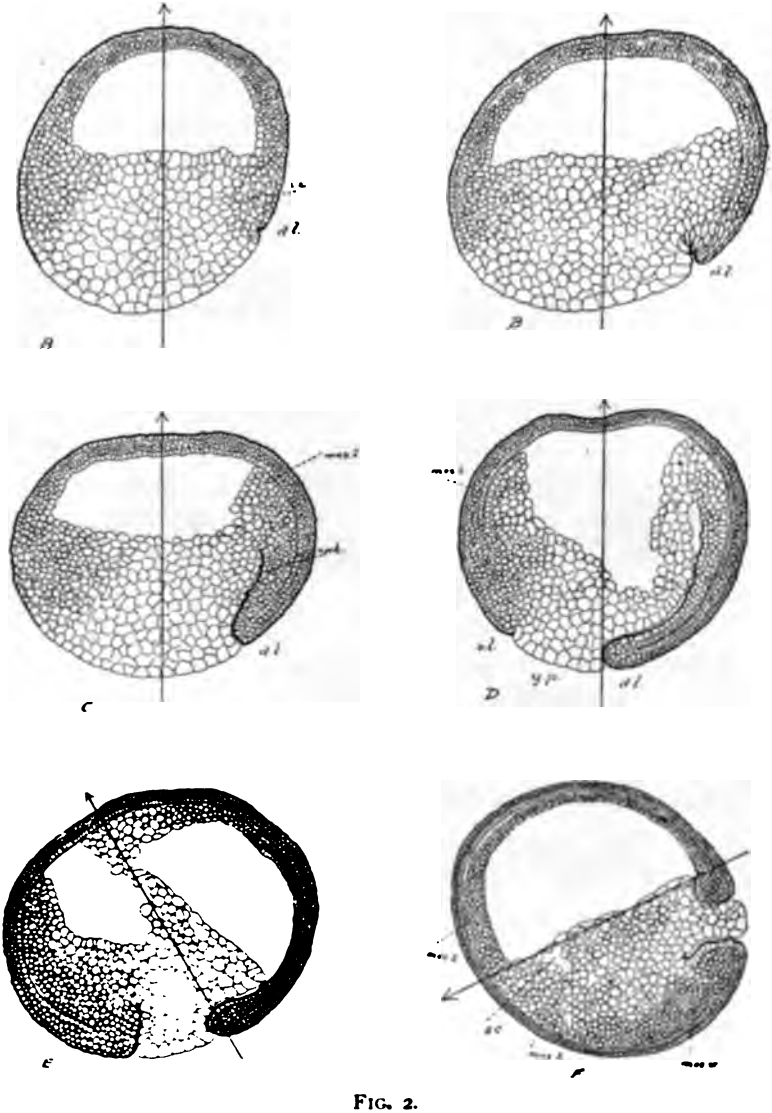

FIG. 2.

Sagittal sections through the Frog's egg during the closure of the blastopore.

A. The dorsal lip just formed: *i.z.* zone of intermediate cells; *d.l.* dorsal lip.

centre of gravity, to account for the rotation in the second case, but no such obvious explanation of the supposed rotation in the first.

The whole of the process which has been described is no more nor less than the closure of the blastopore; the blastopore, which is indeed but a virtual blastopore at the start, being that circular subequatorial line along which the pigmented animal pass into the unpigmented vegetative cells. And clearly this closure is bilateral, taking place as it does most rapidly at the dorsal lip, least rapidly at the ventral lip, and at an intermediate rate at the lateral lips in between. The examination of sections will now show us that the closure involves (1) a movement of the vegetative cells into the segmentation cavity together with (2) an overgrowth and ingrowth of cells at the blastoporic lip resulting in the formation of a new cavity, for which we will retain the time-honoured expression of 'archenteron'; and that during the process the material for the germinal layers is brought into position and laid down.

A sagittal section of the egg passing through the dorsal lip at its first appearance (*Fig.* 2, A) shows the groove placed about 25° below the equator in the zone of intermediate cells. The cells which immediately bound the

B. The dorsal lip further advanced. The yolk cells beginning to creep up into the segmentation cavity.

C. A slit-like archenteron formed (*arch.*).

D. The dorsal lip has reached the vegetative pole; the ventral lip (*v.l.*) is just formed. *Mes.* 2 mesoderm formed from yolk cells pushed into segmentation cavity; *y.p.* yolk plug.

E. The egg is beginning to rotate. The segmentation cavity is reduced, the archenteron enlarged.

F. Rotation completed. *Mes.* 1. mesoderm at ventral lip; *s.* remains of segmentation cavity.

In all the figures the arrow marks the egg axis, the head of the arrow the animal pole.

(Original.)

groove are disposed radially about it; this arrangement marks the beginning of a process of overgrowth and ingrowth which becomes more obvious as development proceeds (*Fig.* 2, B, C, D). It is then seen that a fold of small cells has grown over a certain area of yolk-cells. This fold consists naturally of two sheets, an outer and an inner. The cells of the outer sheet resemble closely the small pigmented cells of the animal hemisphere into which they are uninterruptedly continued; like the latter they are arranged in about four layers, the outermost of which is epithelial. At the lip of the blastopore the outer passes into the inner sheet, the cells in the outermost layer of the former being gradually turned over into the innermost layer of the latter. This inner sheet also consists of several layers of cells, the innermost of which is pigmented and epithelial, the remainder being more irregularly disposed. The inner sheet forms the outer, or, as it will be when the egg has rotated, the upper wall of the slit-like cavity between itself and the yolk-surface which is now covered up. This cavity is the archenteron and the inner sheet of the fold is its roof; its floor is the original vegetative surface of the egg. This overgrowth and ingrowth of cells, with consequent formation of an archenteric cavity, takes place in an exactly similar manner at the lateral (*Fig.* 3, A) and ventral (*Fig.* 2, E) lips; and by the time that this last has appeared the cavity extends over a considerable area of the vegetative hemisphere, as far towards the equator, in fact, as the line from which the lip of the blastopore had originally begun to grow down. Now, however, another process supervenes, namely, an upward movement of the yolk-cells into the segmentation cavity.

The first indication of this is, as a matter of fact, observable at an earlier stage (*Fig.* 2, B). Immediately

over the dorsal lip the yolk-cells at the equator may be observed creeping up into the segmentation cavity, along the under surface of its roof. As this movement begins so it takes place most rapidly on the dorsal side, but it is continued none the less laterally and ultimately ventrally as well. The segmentation cavity is thus reduced to a small space situated ex-axially, and on the ventral side (*Fig.* 2, E). The archenteron meanwhile is extended in the direction of the animal pole (*Fig.* 2, D). This extension may be due to a 'splitting' amongst the yolk-cells, or more probably to an actual invaginatory process;

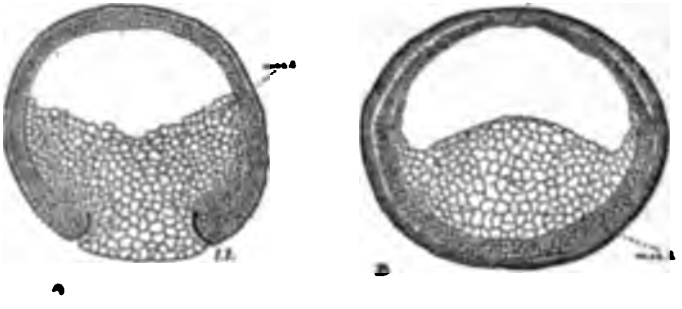

Fig. 3.

Transverse sections of the Frog's egg.

A. Before rotation during overgrowth of the lips of the blastopore. *l.l.* lateral lips. The yolk cells are creeping up into the segmentation cavity.

B. After rotation. The archenteron is widened. *n.ch.* notochord. (Original.)

but in any case a greater change soon follows in its dimensions. The yolk-cells which form its floor are now pushed up towards the animal pole, and inwards towards the egg axis, so that the cavity gains simultaneously in length, depth, and breadth (*Fig.* 2, E, F; and *Fig.* 3, B); it now extends into the animal hemisphere, being separated by only a thin partition of yolk cells from the segmentation cavity. The fate of the latter appears, as Professor Keibel

has informed me, to be variable; in a certain rather small percentage of cases it is stated to fuse with the archenteron; otherwise it is obliterated by further immigration of yolk-cells; traces of it may then be seen below the archenteric floor (*Fig.* 2, F).

The changes just described in the position of the heavy yolk-cells afford the best explanation of the rotation of the egg; for the centre of gravity being shifted to the ventral side, the egg naturally turns about a horizontal axis in the opposite direction.

We are now in a position to discuss the formation of the germinal layers. With the exception of the yolk-plug, the outer surface of the egg is entirely covered by a sheet of small, pigmented cells, disposed in about four layers, the outermost of which is epithelial. In part this sheet consists of the original animal cells which formed the roof of the segmentation cavity; but it is also derived in part from the outer sheet of the fold which grew down at the lip of the blastopore. The sheet in question is of course the ectoderm—as above defined; dorsally it soon becomes thickened to form the medullary plate.

The notochord and mesoderm have a double origin. Antero-ventrally—that is, in the region of the animal hemisphere—they arise from the yolk-cells which have been pushed and folded back into the segmentation cavity. Postero-dorsally—that is, sub-equatorially—they are differentiated in the inner sheet of the fold, the roof of the archenteron; the notochord in the mid-dorsal line in front of the blastopore, the mesoderm in the remainder of the roof around and behind the blastopore. The roof consists of several layers of cells, the innermost or lowest of which is epithelial; the outer or upper layers consist of fairly closely packed polyhedral elements (*Fig.* 4, A). In the middle line (in front of the dorsal lip) a strip or

not passive, since they move into the segmentation cavity, are certainly not in a state of active division, and I think we may say quite confidently that the roof of the archenteron does not come from this source. It consists of cells a little larger than the cells of the animal hemisphere, but resembling the latter closely in being pigmented and in containing few yolk-granules of small size. This, however, is by itself no adequate proof that that is their origin[6]; on the contrary, a careful study of all the stages seems to show that it is the rapidly dividing cells of the subequatorial intermediate zone which principally contributes to the inner sheet of the overgrowth; not, perhaps, entirely though, for inner and outer sheet are continuous at the blastoporic lip, and many of the cells of the latter are probably being perpetually rolled over into the former; the radiate arrangement of the cells seems a sufficient demonstration of this. The outer ectodermal portion of the fold is very largely derived from an extension of the roof of the segmentation cavity; the cells here are certainly in a state of active proliferation, and as the total thickness of the layer does not increase, the total area must necessarily enlarge. This spreading of the animal cells seems indeed to be directly concerned (mechanically) in the first formation of the fold at the blastoporic lip; for if (as, for example, by growing the eggs in certain solutions which affect primarily the yolk-cells and prevent their movement into the segmentation cavity) the closure of the blastopore is experimentally delayed, either the roof of the segmentation cavity at once becomes thrown into numerous wrinkles or the number of cell-layers present in it is increased.

[6] It may be pointed out in this connection that Samassa ("Studien über den Einfluss des Dotters auf die Gastrulation II.", *Arch. Entwickmech.* vol. 2, p. 370–393, 1896) has shown that if in the eight celled stage the four vegetative cells are killed, a typical blastoporic lip and archenteric cavity are subsequently developed from the animal cells alone.

It is clear that this portion of the notochord and mesoderm is brought into position concomitantly with the overgrowth of the blastoporic lip; and that the two sheets of mesoderm are confluent with one another posteriorly in the similarly formed mesoderm behind the ventral lip. The whole of this mesoderm is subequatorial or, after rotation, postero-dorsal. Antero-ventrally the mesoderm and the notochord are completed by a different process; as the yolk-cells are pushed into the segmentation cavity those which lie next the roof of the latter, next the ectoderm that is, may be seen to be in a state of rapid sub-division (*Fig.* 2, C—F: mes. 2) (*Fig.* 3, A); a layer of cells smaller than the yolk but larger than the ectoderm cells is thus produced, placed on what will be the antero-ventral side of the embryo (*Fig.* 3, B), and continuous with the already described postero-dorsal mesoderm around the equator.

The germ layers are thus definitely established; at the lips of the blastopore all three are, of course, continuous with one another. It only remains for us to discuss a little more minutely their origin from the two sets of cells —the so-called primary layers—observable in the egg at the end of segmentation.

A very considerable portion of each germ-layer is actually brought into being during the closure of the blastopore. What is the origin of the cells which form the fold by the overgrowth of which this closure is effected? Are they derived exclusively from the cells of the animal hemisphere alone, as some (notably Lwoff) have maintained; or, while the outer ectodermal sheet is to be traced to the animal cells, is the inner sheet merely due to the invagination of yolk-cells as was first stated by the author of the "Gastraea-theorie"? As far as I can see both these views are erroneous. The yolk-cells though

not passive, since they move into the segmentation cavity, are certainly not in a state of active division, and I think we may say quite confidently that the roof of the archenteron does not come from this source. It consists of cells a little larger than the cells of the animal hemisphere, but resembling the latter closely in being pigmented and in containing few yolk-granules of small size. This, however, is by itself no adequate proof that that is their origin[4]; on the contrary, a careful study of all the stages seems to show that it is the rapidly dividing cells of the subequatorial intermediate zone which principally contributes to the inner sheet of the overgrowth; not, perhaps, entirely though, for inner and outer sheet are continuous at the blastoporic lip, and many of the cells of the latter are probably being perpetually rolled over into the former; the radiate arrangement of the cells seems a sufficient demonstration of this. The outer ectodermal portion of the fold is very largely derived from an extension of the roof of the segmentation cavity; the cells here are certainly in a state of active proliferation, and as the total thickness of the layer does not increase, the total area must necessarily enlarge. This spreading of the animal cells seems indeed to be directly concerned (mechanically in the first formation of the fold at the blastoporic lip; for if (as, for example, by growing the eggs in certain solutions which affect primarily the yolk-cells and prevent their movement into the segmentation cavity) the closure of the blastopore is experimentally delayed, either the roof of the segmentation cavity at once becomes thrown into numerous wrinkles or the number of cell-layers present in it is increased.

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To sum up, the ectoderm of the Frog is derived from the cells of the animal hemisphere, the endoderm is derived partly from the intermediate cells, partly from the yolk-cells of the vegetative hemisphere ; while mesoderm and notochord have a similar double origin. It is during the closure of the blastopore—the subequatorial bounding line between the animal and the vegetative cells—that all three layers are brought into their definitive positions, and this closure is bilaterally symmetrical, taking place most rapidly at the dorsal lip. A similar statement may be made of the process of germ-layer formation in the remaining Anamnia.

CYCLOSTOMATA.

Petromyzon.

The egg of the lamprey, like that of the frog, is micro-lecithal and holoblastic. At the close of segmentation it presents essentially the same characters, namely, an animal hemisphere of small cells, which form a roof, and a vegetative hemisphere of large yolk-cells, forming a floor for the excentric segmentation cavity.

The dorsal lip of the blastopore now appears and the blastopore closes bilaterally as in the frog ; we have not, however, in the case of the lamprey that exact knowledge of the angle traversed by the dorsal lip. There is further a very important difference between the two in that here no ventral lip is formed (*Fig.* 5, A). Apart from that the result appears to be closely similar in the two cases. The overgrowth and ingrowth of cells produces an archenteron which is said to open into the remains of the segmentation cavity, and the material for the three layers is at the same time brought into position. The ectoderm in its mode of origin resembles that of the frog ; the mesoderm seems here also to be derived from two sources (*Fig.* 5, B),

partly from cells carried back with the overgrowth, in part from yolk-cells pushed into the segmentation cavity ; this at least is the account given by Scott ; other observers—Gotte, Kupffer, Shipley—have attributed it to either the one or the other alone. The fate of the roof of the archenteron is, however, by no means identical ; whereas in the frog, as we have seen, this layer becomes differentiated into notochord and mesoderm above, and the roof of the definitive alimentary tract below, its destiny in the lamprey is to give rise to the notochord and the

FIG. 5.

Petromyzon (after Scott).

A. Sagittal section after the overgrowth of the blastoporic lip; *d l*, dorsal lip; *ar. h*., archenteron.

B, *C*. Transverse sections showing formation of dorsal (*d. m.*) and ventral (*v. m.*) mesoderm, and notochord (*n. ch*.); *m. t*. (solid) medullary tube.

notochord alone. The archenteron is here a very narrow cavity, and its roof proportionately small. A median groove appears running along the ventral side of the roof, and the whole becomes folded off and lifted out from the side walls of the archenteron as a strip, subsequently a rod of cells, the notochord (*Fig.* 5, C). The lateral walls then grow in towards the middle line, and, meeting, complete the roof of the gut. The endoderm, therefore, does not—as far as its mode of origin is concerned—wholly and exactly correspond in these two types.

Bdellostoma.

Our knowledge of the early phases of development in the megalecithal and meroblastic Myxinoids is exceedingly meagre, but Bashford Dean has shown that in

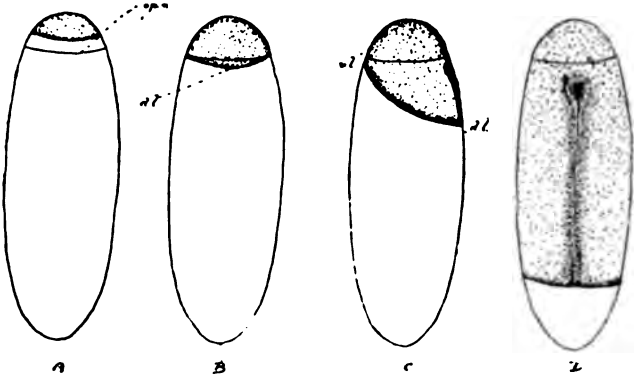

FIG. 6.

Baellostoma (after Dean).

A. Blastoderm at one pole of the ellipsoid egg; *op.r.*, opercular ring of the shell.

B. Dorsal lip (*d.l.*) beginning to grow down.

C. A little later: *v.l.*, ventral lip.

D. Embryo formed: blastopore closing at vegetative pole.

Bdellostoma the egg axis is identical with the major axis of the ellipsoid egg, and that segmentation produces a cap of cells or blastoderm[7] at one pole (*Fig.* 6, A). At one point in the edge of this blastoderm a dorsal blastoporic lip appears, and the material for the germ-layers of the embryo is laid down during the bilateral overgrowth and

[7] Throughout this paper I have followed the customary usage in employing one term, "blastoderm," to denote the cap of cells produced by segmentation at the animal pole of large-yolked eggs, both in the Anamnia and Amniotes; and having regard to the similarity in origin of the cap of cells in question in the two cases the use of a common term is justifiable. In their subsequent history, however, as will be fully shown below, the Anamnian and the Amniote "blastoderms" differ widely; and it might therefore be advisable to retain the expression for one of these only if a suitable word could be found for the other.

ingrowth of cells in this region (*Fig.* 6, B, C). The yolk, however, is not wholly covered by this process. As soon as the body of the embryo is formed overgrowth ceases to be bilateral; all parts of the edge of the blastoderm—that is, the lip of the blastopore, now take part in the process, and the blastopore eventually closes at the vegetative pole (*Fig.* 6, D).

It is worth while noticing that in this case, where, since the egg is ellipsoid, the egg-axis can always be recognised, there cannot be the least doubt that the movement of the lip of the blastopore really involves an overgrowth, and is not due to a rotation of the egg as a whole.

ELASMOBRANCHII.

The Elasmobranch ovum is megalecithal and meroblastic. At the end of segmentation there is found at the animal pole a blastoderm lying on and continuous at its edges with the unsegmented yolk. The blastoderm consists of a superficial upper layer of columnar cells arranged in an epithelium and a mass of loosely connected lower layer cells lying in the cavity—the segmentation cavity—which separates the upper layer from the yolk. The lower layer cells are disposed principally in two groups; a group occupying almost the whole vertical extent of the segmentation cavity in the anterior region of the blastoderm, and a smaller group close to the margin of the blastoderm at the posterior end (*Fig.* 9, A). Below the segmentation cavity is the yolk, unsegmented but nucleated. Many of these yoke-nuclei are exceedingly large, and a good number are, according to Ruckert, derivatives of accessory spermatozoa. Though small cells are continually segmented off from the yolk to be added to the lower layer cells it is doubtful whether any of the yolk-nuclei play any part in the formation of the embryo;

those which remain in the yolk are probably concerned solely with facilitating its liquefaction and absorption.

There is now produced at the edge of the blastoderm posteriorly and in the middle line a fold or overturning of cells of the superficial layer. The fold, which is the dorsal lip of the blastopore, is slightly raised and covers over a space—the beginning of the archenteron—between itself and the yolk. By the continuation of the backward growth of the fold (*Fig.* 9, B, C) (the whole blastoderm is also

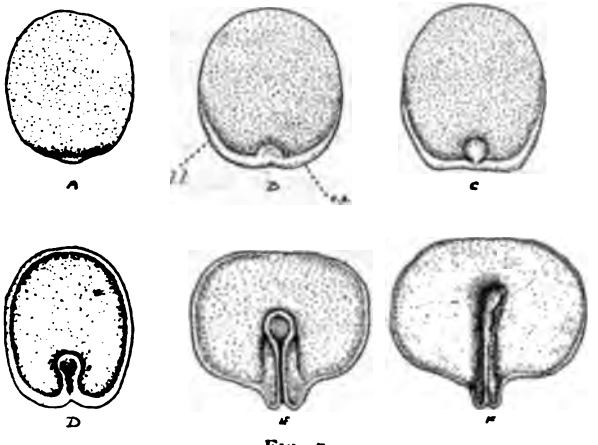

FIG. 7.

Torpedo. *A*—*C* after Rückert, *D*—*F* after Ziegler (slightly modified).

A. Formation of the dorsal lip at the posterior end of the blastoderm.

B, *C*. Formation of the lateral lips (*l.l.*) and caudal swellings (*c.s.*). The extent of the archenteron, crescentic with a median prolongation, is indicated by the shading.

D, *E*, *F*. Extension of the blastoporic lip to the anterior edge of the blastoderm. Formation of the embryo in the hinder region.

steadily spreading over the yolk) the archenteron attains a considerable length ; its floor is formed of yolk into which yolk-nuclei subsequently make their way ; its roof consists of a columnar epithelium derived in part from the overturning of cells at the lip of the blastopore, in part possibly from the posterior marginal lower layer cells. But while

this process is taking place at the dorsal lip, that is, at the median posterior region of the blastoderm's edge, it is also being extended, though in a far less degree, to the neighbouring regions, the lateral lips, on the right and left (*Fig* 7, A, B). The archenteron thus comes to assume a

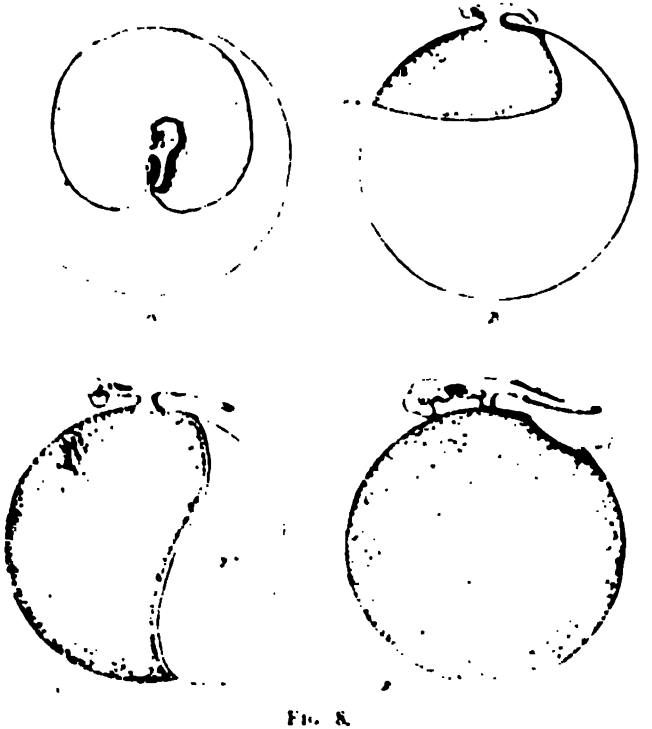

FIG. 8.

Growth of the Elasmobranch blastoderm over the yolk and final envelopment of the latter; *a v.* anterior edge which becomes *l.* ventral lip; *y* yolk blastopore. In *A* the median strip of yolk behind the dorsal lip is seen bounded by the two lateral lips.

crescentic shape, with a median anterior prolongation; the latter underlies the embryonic portion of the blastoderm, the crescentic part is wholly extra-embryonic, and remains very shallow, though it is subsequently prolonged to the right and left round the edges of the blastoderm

until a slight overgrowth is formed even at the anterior margin.

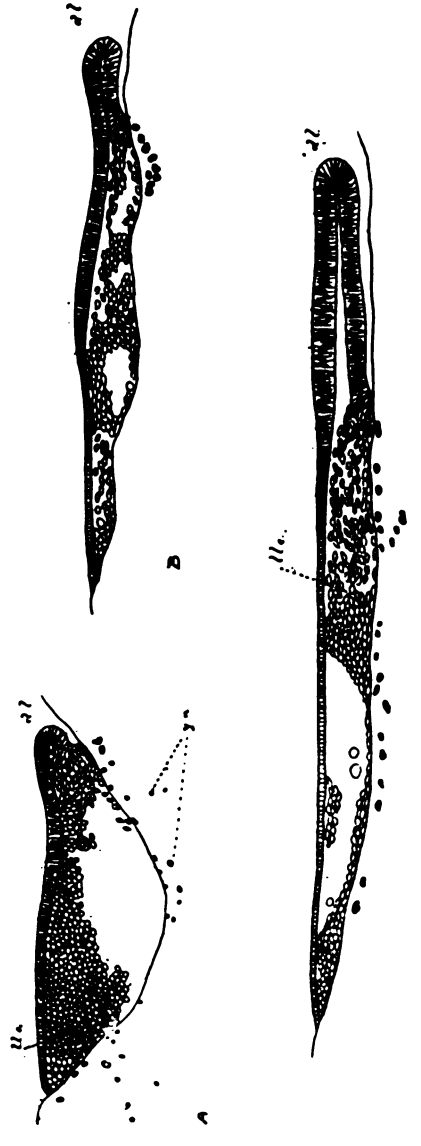

FIG. 9.

Sagittal sections of the blastoderm of Elasmobranchs during the formation and backward growth of the dorsal lip. *A. Pristiurus. B, C. Torpedo* (after Rückert).

A. The dorsal lip is just formed (*d. l.*); *l.l.c.* lower layer cells; *y.n.* yolk-nuclei.

B, C. Two later stages. The whole blastoderm has grown in length, and the dorsal lip, growing backward, has produced an archenteron between itself and the yolk. Letters as before.

With the overgrowth at the lips of the blastopore the material for the germ-layers is laid down. The superficial layer is now the ectoderm. The mesoderm consists

FIG. 10.

A series of transverse sections through the blastoderm of *Scyllium* in the stage represented in *Fig.* 7 *C* to show the formation of notochord and mesoderm. (Original.)

A passes through the caudal swellings: the mesoderm is seen arising from the roof of the archenteron.

B. Just in front of the dorsal lip. Two mesoderm plates are now separated from the roof of the archenteron except at the lateral lips (*l.l.*). Medially the roof of the archenteron is thickened for the notochord.

C. Further in front. The archenteron is divided into a median embryonic portion and two lateral portions beneath the lips of the blastopore.

D, *E*. The notochord is cut out of the roof of the archenteron; the mesoderm is separated into two median embryonic (*e.m.*) and two lateral extra embryonic (*ex.m.*) sheets.

of two parts (*Fig.* 10), (1) two sheets of cells lying one on each side of the median line over the embryonic portion

of the roof of the archenteron; posteriorly these sheets pass into the caudal swellings—two thickenings at the edge of the blastoderm, one on each side of the middle line (*Fig.* 7, B, C; *Fig.* 10, A)—where they fuse with the roof of the archenteron, out of which they have gradually been differentiated; (2) the formation of mesoderm is, however, not limited to the dorsal lip, but is carried on at the lateral lips, and, as these extend forwards round the whole margin of the blastoderm, at the anterior edge as well.

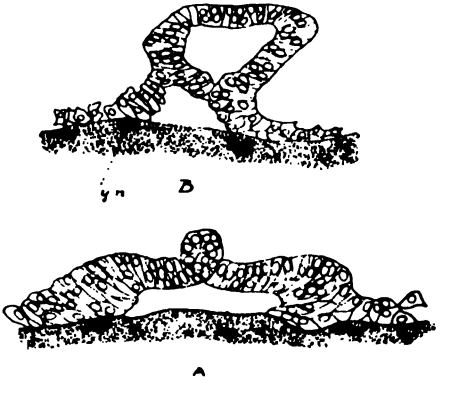

FIG. 11.

Two stages in the ventral closure of the gut in *Scyllium*. (Original.)

In *B* the ventral wall is seen to be formed by suture of the sides of the archenteron: yolk and yolk-nuclei (*y.n.*) below.

This extra embryonic mesoderm ('peristomales' of Rabl) is naturally continuous in the caudal swellings with the embryonic ('gastrales') mesoderm first described; it takes part only in the formation of the area vasculosa. The notochord is formed from a median strip of cells which is cut out of the roof of the archenteron (*Fig.* 10, D, E; *Fig.* 11, A); the process begins in front and proceeds backwards. The exact origin of the lining epithelium of the alimentary canal—the endoderm—is by no means so

certain. When the embryo begins to be folded off from the hinder middle region of the blastoderm, the roof of the archenteron is involved in the folding, and forms at least the roof of the gut; the floor, according to Balfour, is derived from a layer of cells formed round the nuclei of the underlying yolk. I must say that my own preparations do not support this view; the yolk-nuclei are much larger than, and totally unlike, any of the nuclei of the blastoderm; and it seems to me perfectly clear that the gut is closed ventrally by suture of the sides of the archenteron as this becomes folded off (*Fig.* 11, A, B).

Up to the present it is the posterior edge or dorsal lip which has been principally active, but now growth ceases at that point, elongation of the embryo being effected by the independent proliferation of the caudal swellings (*Fig.* 7, F). The anterior and lateral margins of the blastoderm, on the contrary, become exceedingly vigorous and begin to grow over the yolk, the overgrowth being accompanied, as stated above, by a slight marginal invagination; and eventually the anterior edge makes the whole circuit of the yolk, passing round the vegetative pole and reappearing behind the embryo as the ventral lip of the small 'yolk-blastopore' (*Fig.* 8, B - D). A peculiar change has also taken place on each side of the dorsal lip, the lateral lips immediately adjacent to it having swung back until they bound a narrow median strip of yolk (*Fig.* 8, A), by which alone the aperture at the dorsal lip now communicates with the remainder of the blastopore.

It is now possible for us to institute a series of comparisons between the processes we have observed in the microlecithal and macrolecithal types. These comparisons are as follows:—

(1) The blastoderm of the Elasmobranch corresponds

to the animal hemisphere of the frog's egg, the yolk to the vegetative hemisphere.

(2) Hence the edge of the blastoderm in the former is equivalent to the subequatorial bounding line between animal and vegetative hemispheres in the latter.

(3) In both this bounding line becomes in its entirety the lip of the blastopore, the posterior point of the edge in the Elasmobranch being the dorsal lip of the frog, the anterior point of the edge the ventral lip.

(4) In both the blastopore closes bilaterally, growth being greatest at the dorsal lip.

(5) In both the material for the germinal layers is laid down during the process; the notochord lies in front of the dorsal lip, the mesoderm at the lateral lips in two sheets continuous with one another behind in the ventral lip.

The principal points of difference are two. First, in the Elasmobranchs the closure of the blastopore is divided (as in *Bdellostoma*, and also, as we shall see in a moment, in the Teleostei) into two periods; in the first, the overgrowth is almost confined to the dorsal lip and produces the material for the formation of the embryo; in the second, the yolk is gradually covered by an extension of the blastoderm in which the lateral and anterior margins are alone concerned. Secondly, in the Elasmobranch a part only of the blastoporic lip is involved in the formation of the embryo, the lateral and ventral lips remaining wholly extra-embryonic.

TELEOSTEI.

The egg of the Teleostei is, like that of the Elasmobranchs, megalecithal and meroblastic. Segmentation

leads to the production of a blastoderm lying upon, but not continuous with, the yolk. In the latter are numerous nuclei derived from the blastoderm in an earlier stage, and, as a rule, from its edge, though occasionally (Salmonidae) from the whole of its lower surface. These

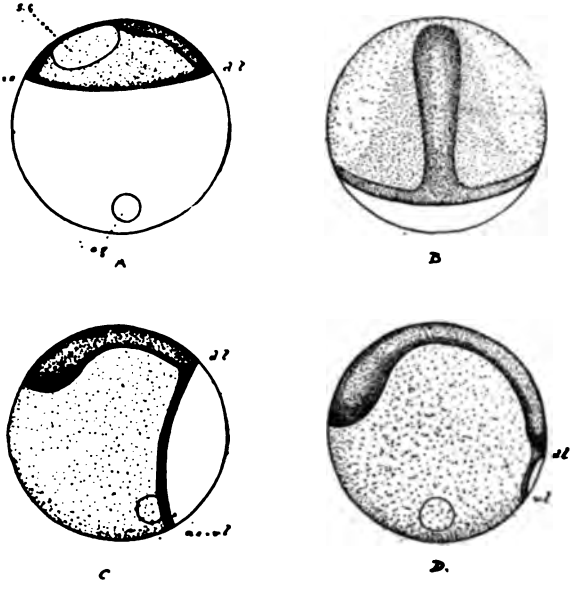

FIG. 12.

Formation of the embryo and closure of the blastopore in *Serranus* (after Wilson).

A, *B*. The material for the embryo is laid down by overgrowth at the dorsal lip (*d.l.*) *A*, from the left side; *B*, from above; *s.c.*, segmentation cavity; *o.g.*, oil globule; *a.e.*, anterior edge of the blastoderm.

C, *D*. Final envelopment of the yolk by growth mainly of the anterior edge, or ventral lip (*v.l.*).

The shading at the edge of the blastoderm indicates the formation of extra-embryonic mesoderm.

nuclei lie in a superficial clear layer of the yolk, the periblast; they do not in any wise contribute to the formation of any part of the embryo, but subserve simply the liquefaction and elaboration of the yolk.

Several accounts have been given of the process of germ-layer formation in this group; the best is, perhaps, Wilson's work on the "Embryology of the Sea-Bass" (*Serranus*). In *Serranus* the blastoderm consists of a

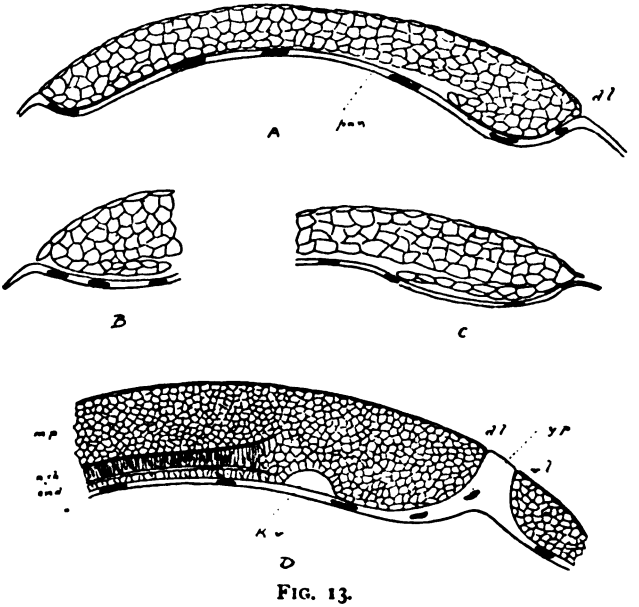

FIG. 13.

Serranus (after Wilson).

A. The blastoderm lying on the nucleated periblast (*par.*) is cut sagittally. Ingrowth of cells is just beginning at the dorsal lip (*d.l.*).

B. Ingrowth of cells at the anterior edge (ventral lip).

C. The same at one of the lateral lips.

D. Sagittal section after complete enclosure of the yolk; *k.v.* Kupffer's vesicle; *y.p.* yolk-plug; *m.p.* medullary plate; *n.ch.* notochord; *end.* endoderm; other letters as before.

superficial epidermic layer of flat cells, covering three or four layers of closely packed polyhedral cells below. There is a slight segmentation or subgerminal cavity between it and the yolk.

At one point—the posterior middle point—of the edge of the blastoderm a thickening, followed by an

overgrowth and ingrowth of cells, takes place (*Fig.* 13, A); the flat epidermic layer is not included in the process. This point is, of course, the dorsal lip, the ingrowing sheet of cells the roof of the archenteron ;[6] the latter consists of two or three cell layers and terminates anteriorly with a free margin. The ingrowth proceeds until sufficient material has been laid down for the embryo, and the archenteron has reached a good length. Meanwhile, a similar, though much smaller, ingrowth has been taking

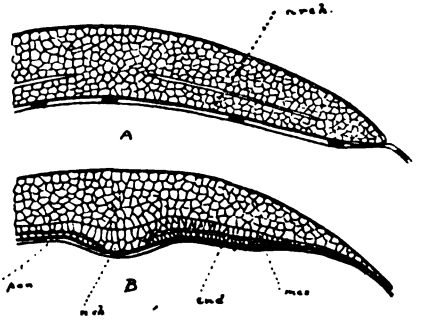

FIG. 14.
Serranus (after Wilson).
Transverse sections of the embryonic region.
A. The roof of the archenteron (*arch.*) is undifferentiated.
B. The roof of the archenteron is differentiated into notochord (*n.ch.*) in the middle line, and mesoderm (*mes.*) and endoderm (*end.*) at the sides.

place round the whole of the periphery of the blastoderm ; this portion is merely extra-embryonic mesoderm (*Fig.* 13, B, C). When the process has, with the formation of the embryo, come to an end at the dorsal lip, the remainder of the edge of the blastoderm continues to grow with its lip-like edge over the surface of the yolk until this is completely enveloped ; the anterior edge, however, travelling faster than the lateral portions, sweeps round the vegetative pole and appears behind the embryo

[6] Wilson inclines to the view that the roof of the archenteron is produced by delamination as the thickened dorsal lip grows backwards.

as the ventral lip of the circular blastopore (*Fig.* 12) (*Fig.* 13, D). The closure of the blastopore therefore falls into the same two periods and takes place in essentially the same manner as it does in the Elasmobranchs.

Let us now return to the formation of the germ-layers in the embryonic region. The superficial portion of the blastoderm is the ectoderm. The endoderm, as well as the notochord and mesoderm, is contained in that sheet of cells—the roof of the archenteron—which has grown forwards beneath the dorsal lip (*Fig.* 14, A). In this sheet two narrow slit-like cavities presently

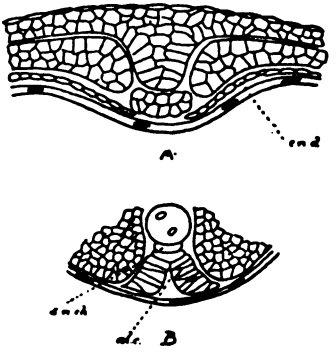

FIG. 15.

Serranus (after Wilson).

Formation of the alimentary canal (*al.c.*) by fusion, and folding of the two endodermal plates (*end.*); *s.n.ch.* sub-notochordal rod.

appear, one on each side of the middle line; each cavity is parallel to the surface of the sheet, and soon extends outwards to its edge; the whole sheet is thus split on each side with an upper and a lower plate, a median tract remaining undivided (*Fig.* 14, B). This middle strip then separates as the notochord from the two lateral portions. The latter consist now each of an upper meso-

dermal and a lower endodermal plate. To complete the alimentary canal (*Fig.* 15) the two plates of endoderm first fuse by their inner edges below the notochord; the outer edges then bend down, meet, and so close the central wall of the gut.

A variation in the mode of formation of the notochord, recalling the similar difference between the frog and the lamprey, has been described by Henneguy in the trout; here the split in the roof of the archenteron passes right across the middle line; two cell plates are thus formed, a lower, the endoderm, and an upper, which then becomes differentiated into a median notochord and two lateral mesodermal sheets.

It is clear that the close similarity which obtains between the Teleostei and the Elasmobranchii, in respect of the closure of the blastopore, extends also to the mode of formation of the germinal layers.

GANOIDEI.

Our knowledge is here unfortunately very slight.

From the observations of Sobotta, Whitman and Eycleshymer, and Bashford Dean, we know that in *Amia*, *Lepidosteus*, and *Acipenser* the segmentation of the ovum

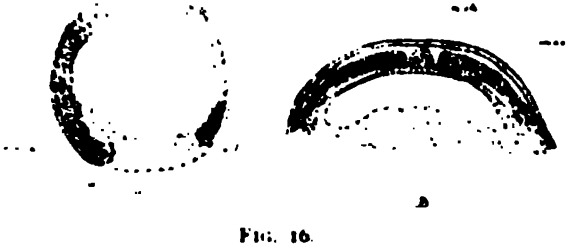

FIG. 16.

Formation of the germ layers in *Acipenser* (after Dean).

A. Sagittal section. *B.* Transverse section of dorsal half of embryo. Lettering as before.

is intermediate between the holoblastic and the meroblastic types: the yolk becomes but very slowly divided.

The closure of the blastopore—during which the material for the notochord and mesoderm is brought into

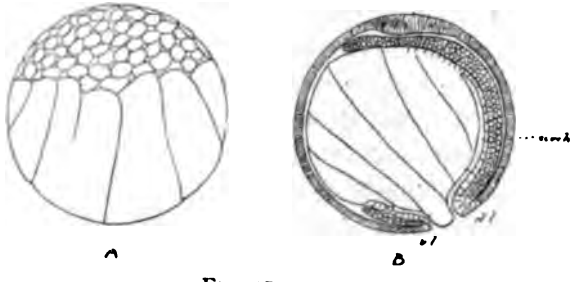

FIG. 17.

Formation of the germ-layers in *Amia* (after Sobotta).

A. Axial section of the egg at the close of segmentation; the yolk only partially segmented.

B. Sagittal section after closure of the blastopore. Letters as before.

position and the archenteron formed—is bilaterally symmetrical, exhibiting the usual preponderance of overgrowth at the dorsal lip. The blastopore becomes completely circular, and is provided with a ventral lip (*Figs.* 16, 17).

DIPNOI.

With regard to the Dipnoi we are, however, thanks to the researches of Graham Kerr and Semon, better informed.

In *Ceratodus* the egg is holoblastic. Closure of the blastopore is bilaterally symmetrical, there is a ventral as well as a dorsal lip, and a spacious archenteric cavity is formed, which does not communicate with the segmentation cavity. The roof of the archenteron consists of a broad cell plate (*Fig.* 18, A) which becomes divided into a median strip, the notochord, and two lateral mesodermal sheets. The vegetative cells, which line the

floor of the archenteron, grow up beneath the mesoderm, and, fusing in the middle line below the notochord,

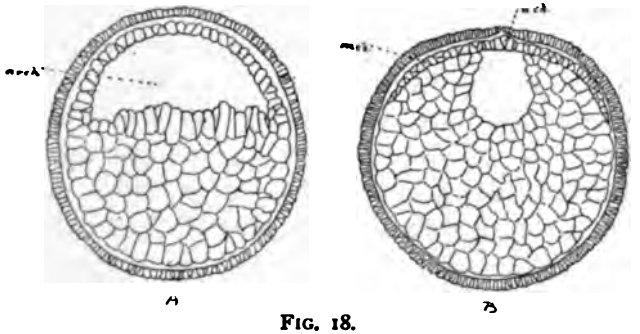

FIG. 18.

Ceratodus (after Semon).

A. Transverse section, showing wide archenteron.

B. The plate of cells forming the roof of the archenteron is being undergrown by yolk-cells from the sides.

Lettering as above.

complete the dorsal wall of the alimentary canal (*Fig.* 18, B).

In *Lepidosiren* on the other hand, the egg inclines towards a meroblastic segmentation. The blastopore

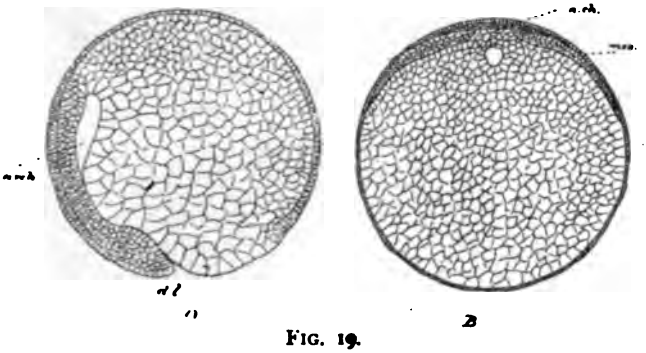

FIG. 19.

Lepidosiren (after Graham Kerr).

A. Sagittal section during overgrowth of dorsal lip (*d.l.*), and formation of archenteron (*arch.*). Note that there is no ventral lip.

B. Transverse section, showing dorsal mesoderm (*mes.*) brought into position with the overgrowth of the lip of the blastopore, and notochord (*n.ch.*) split off the roof of the archenteron.

closes bilaterally by an overgrowth of the usual character, but there is no ventral lip (*Fig.* 19, A); on this side the area of small animal cells is extended by delamination from the yolk. Dorsally the mesoderm is carried into place during the overgrowth of the blastoporic lip; ventrally it is differentiated from the vegetative cells (*Fig.* 19, B). The notochord is split off the roof of the archenteron (which then becomes the gut), as in the frog, which this type indeed closely resembles in all respects, except in the amount of yolk and the absence of the ventral lip.

AMPHIBIA.

Urodela.

In the Urodela, the process of germ-layer formation

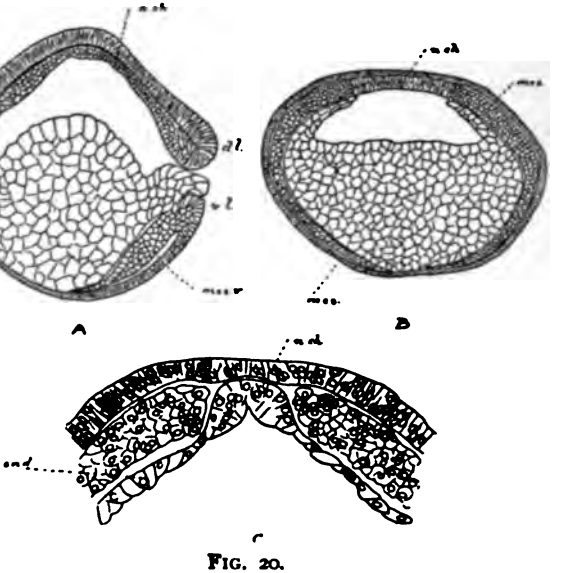

FIG. 20.

Formation of the germ-layers in the *Axolotl.*

(Original.)

A. Sagittal section at the time of closure of the blastopore.

B. Transverse section of the same stage. Letters as before.

C. Transverse section at a later stage, to show the conversion of the whole of the archenteric roof into notochord. The lateral yolk-cells are growing in towards the middle line.

is practically identical with that which is observed in the Anura, except in one important respect. In the bilateral closure of the blastopore, the presence of a ventral as well as a dorsal lip (*Fig.* 20, A) and the formation of the mesoderm from a double source, the two groups closely resemble one another; but while in the frog the under layer of the roof of the archenteron persists as the dorsal lining of the digestive tract, in the Urodeles the sheet of cells in question becomes wholly converted with the upper layers of the roof into the notochord, just as is the case in *Petromyzon*, and the roof of the gut is formed by an ingrowth of vegetative cells from the sides (*Fig.* 20, B, C).

Gymnophiona.

The last group of the Anamnia whose germinal layers remain to be considered is the Gymnophiona. We here follow Brauer's excellent description of *Hypogeophis*.

In the Gymnophiona the egg is so laden with yolk that it nearly approaches the meroblastic type, and the result of segmentation is what may fairly be described as a blastoderm resting on a partially divided yolk. The blastoderm comprises a superficial epithelium of columnar cells, thinner and lower at the margin, and covers several irregular layers of scattered cells, which are more abundantly supplied with yolk. The cavities between these cells are equivalent to the ordinary segmentation cavity (*Fig.* 22, A). Below the segmentation cavity is the yolk, divided at its surface into cells, and containing scattered nuclei throughout its substance. Immediately round the blastoderm the surface of the yolk is also partially segmented.

At one point—the posterior middle point—of the edge of this blastoderm, the dorsal lip appears; it exhibits the usual radial arrangement of cells (*Fig.* 22, A).

The dorsal lip quickly grows back, and produces a long archenteron, which at its anterior end opens into the segmentation cavity. The roof of the archenteron, which seems to be derived entirely from the superficial layer of the blastoderm, consists of a plate of columnar cells, its floor of the partially segmentedyolk (*Fig.* 22, B).

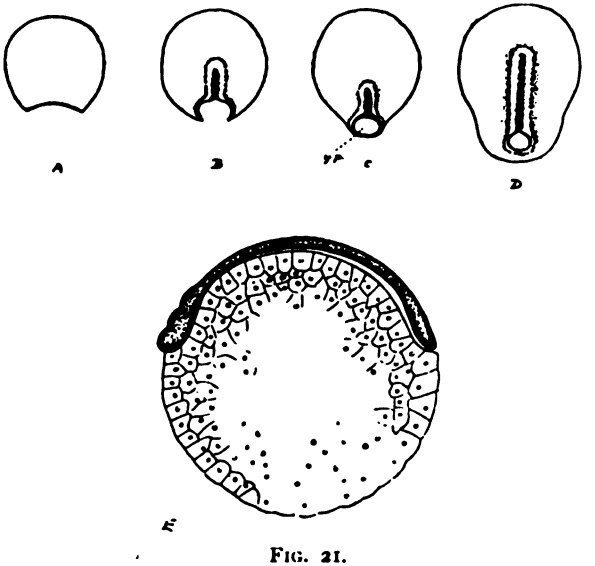

FIG. 21.

Gymnophiona.

A—D. Four successive stages in the closure of the blastopore and formation of the ventral lip in *Hypogeophis* (after Brauer). The blastoderm only is shown ; *y.p.* yolk-plug.

E. Diagrammatic section of the egg of *Ichthyophis* (after Sarasin) to show the embryo resting on a partially segmented and unenclosed yolk.

The process of overgrowth is of course not limited to the dorsal lip, but extends to the immediate right and left. Surface views show that the transverse groove, the outward sign of this lip, soon becomes crescentic ; the horns of the crescent then grow not only backwards, but towards the middle line as well, approaching one another

dorsal lip, where they are perfectly continuous with the sheets of mesoderm produced at the sides of and behind the blastopore. The mesoderm thus exhibits all the relations which it has in the Anamnia.

The lining epithelium of the alimentary canal is derived from the paraderm, which grows in from the sides beneath the mesoderm and notochord towards the middle line. The gut is subsequently folded off from this layer in the ordinary fashion.

The notochord and mesoderm may therefore be said to be laid down in the Reptilia, as we have seen them to be in the Anamnia, at the lips of and during the closure of the blastopore, a closure moreover which is as bilaterally symmetrical here as it is there.

We have only now to consider one or two interesting points in some other forms.

An embryonic shield distinct from the surrounding blastoderm has been observed in most cases (*Cistudo*, *Emys*, *Trionyx*, *Lacerta*, *Tropidonotus*, *Crocodilus*).

A primitive plate is found in *Lacerta* and *Tropidonotus*, but not in *Emys*.

In *Lacerta*, according to Wenckebach, the lip of the blastopore is not the only source of origin of notochord and mesoderm; both receive additions in front by the proliferation of cells of the anterior paraderm. A similar double origin of the middle layer has been described in several other forms; it may be compared with the double origin of the mesoderm in *Petromyzon*, the Frog and some other Anamnia.

In *Emys* on the other hand the paraderm takes no part in the formation of any portion of the embryo whatever, but gives rise to the lining of the yolk-sac alone. The roof of the archenteron in this form splits into two sheets, an upper, which again becomes sub-divided into

plug (*Fig.* 22, C, D). The resemblance of a section through the blastopore to a section through the circular blastopore of a frog or newt is in fact complete. If the section were continued, however, through the entire egg a very striking discrepancy would be at once apparent, a discrepancy which depends on the difference in the mode of formation of the ventral lip.

In other Amphibia the ventral lip is developed from that point in the subequatorial transition zone which is diametrically opposite to the dorsal lip, and the whole of the vegetative surface of the egg becomes consequently covered up when the blastopore closes. Here, on the contrary, the anterior edge of the blastoderm and a large part of the lateral edges have no share whatever in the ventral lip, which arises entirely by the fusion of the extremities of the two lateral lips; as a result the vegetative hemisphere remains entirely uncovered. This is shown very well in Sarasin's figure of *Ichthyophis* (*Fig.* 21, E). In other words while in the Anura and Urodela (and all other Anamnia) the whole of the edge of the blastoderm becomes converted into a blastoporic lip, the posterior point being the dorsal, the anterior becoming sooner or later the ventral lip, in the Gymnophiona only a small posterior region of the blastoderm's margin ever becomes active in this way, and this small portion gives rise to the dorsal and the two lateral lips, which latter by their fusion produce such a remarkable similitude of the ventral lip of the other forms. The importance of this fact for the correct understanding of the relations of the blastopore to the blastoderm in the Amniota cannot possibly be overestimated.

To return to the germinal layers. The superficial layer of the blastoderm is now of course the ectoderm. The plate of cells which forms the roof of the archenteric

cavity becomes divided, as it does in *Ceratodus*, into three portions ; a narrow median strip, the notochord, and two lateral sheets of mesoderm (*Fig.* 23, B) which are continuous with one another behind the yolk-plug by the cell plate invaginated at the lateral and ventral lips. The mesoderm has in fact precisely the same relations as in the frog (or other Anamnian) at a similar stage.

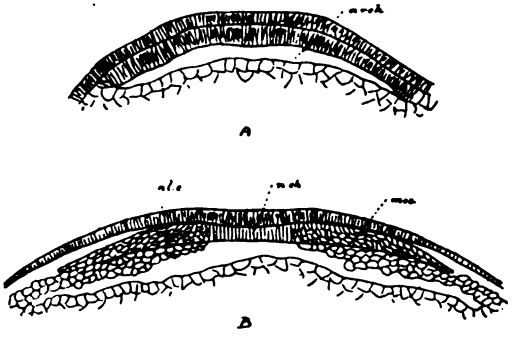

FIG. 23.

Hypogeophis (after Brauer).

Differentiation of the roof of the archenteron into notochord and mesoderm ; formation of the roof of the definitive gut (*al.c.*) by ingrowth and undergrowth of yolk-cells.

Brauer states expressly that no additions are made to either the notochord or the mesoderm from any other source.

The roof of the gut is completed by upgrowth and ingrowth of vegetative cells below the middle layer (*Fig.* 23, B).

Let us now, before proceeding to the Amniota, endeavour to express, in as brief a form as possible, the principal facts of germ-layer development as they occur in the Anamnia.

It appears that—

(1) In all cases the edge of the blastoderm (applying this term to both microlecithal and megalecithal forms) becomes in whole or in part the lip of the blastopore. In all cases the posterior edge becomes the dorsal lip, in all cases except the Gymnophiona the anterior edge becomes the ventral lip (unless this is absent).

(2) The blastopore closes bilaterally; the closure involves an overgrowth and ingrowth of cells which is most vigorous at the dorsal, less vigorous at the lateral, and least vigorous at the ventral lip.

(3) During this closure an archenteric cavity is formed and the material for the ectoderm, the notochord and the mesoderm, and the roof of the archenteron is brought into its definitive position. The notochord lies along the median line in front of the dorsal lip, the lateral mesoderm sheets pass into one another behind the ventral lip. All three sets of cells ectoderm, notochord and mesoderm, and roof of archenteron are continuous with one another at the lips of the blastopore.

(4) Distinct discrepancies are observable in the manner in which sets of cells which resemble one another in origin are used in the production of the germinal layers; this is particularly the case with the roof of the archenteron, which may form the whole, or only a part of, or be absolutely excluded from the definitive alimentary tract.

We may now turn to the Amniota.

AMNIOTA.

Whereas in the Anamnia the blastoporic lip is formed at the edge of the blastoderm in the Amniota the blastopore lies wholly within the latter. The Amniote blastoderm[9] consists of two layers an upper and a lower (frequently termed epiblast and hypoblast and identified with the definitive ectoderm and endoderm). Well within the margin of this blastoderm a blastopore (primitive groove) is formed leading into an archenteron ; concomitantly the material for the germ-layers is laid down. All these structures are in many cases derived in the first instance solely from the upper layer, though eventually a connection with the lower layer is set up. The edge of the blastoderm, which is entirely independent of the blastopore, grows steadily over the surface of the yolk finally enclosing it at the vegetative pole.

The whole process is far clearer in the Reptiles than in either of the other two groups. They will accordingly be considered first.

REPTILIA.

The principal authorities for the early development of the Reptilia are Will, Wenckebach, Mehnert, Mitsukuri, and Ballowitz. The best account is perhaps Will's description of the formation of the layers in *Platydactylus*, a Gecko.

There is distinguishable in the blastoderm at the close of segmentation a circular or oval area placed excentrically towards the posterior end ; this area is the embryonic shield (*fig.* 24). The blastoderm consists of two layers, an upper and a lower ; the upper layer consists of cylindrical cells in the embryonic shield, of flat cells in the surrounding region ; below it is the segmentation cavity.

[9] See Note 7, page 18.

The lower layer is an irregular sheet of scattered rounded cells, not arranged at present in an epithelium, and is constantly being reinforced by the addition of cells from the nucleated yolk beneath (*Fig.* 25, A). For the lower layer I propose—in order to avoid any morphological implications—to employ the term "paraderm," first suggested by Kupffer. Between the paraderm and the yolk is a shallow cavity, the subgerminal cavity. At one point in the posterior margin of the embryonic shield the upper layer and the paraderm are continuous; this point of fusion is termed by Will the primitive plate (*Fig.* 25, A, *pp.*).

The paraderm cells next arrange themselves in a flat epithelium (*Fig.* 25, B). At the same time a depression makes its appearance in the primitive plate; the anterior margin of this depression is the dorsal lip of the blastopore.

Seen from the surface the dorsal lip presents the appearance of a transverse groove at the hinder margin of the embryonic shield. The groove rapidly becomes crescentic, the horns of the crescent grow back, meet, and fuse behind the primitive plate, which now corresponds exactly to the Amphibian or rather to the Gymnophionan yolk-plug (*Fig.* 24, A—C). A study of sections brings out the similarity more clearly still.

Beneath and in front of the dorsal lip there is produced by invagination of the upper layer of cells a cavity which rapidly increases in length until it reaches the anterior end of the embryonic shield (*Fig.* 25, C, D); the cavity is proportionately broad (*Fig.* 27, A). The roof consists of a layer of columnar cells which at the dorsal lip turn over in the ordinary way into the cells of the upper layer. The floor is now, even in the region of the primitive plate, perfectly distinct from the underlying paraderm. In

A B C

FIG. 24.

Three stages in the formation and closure of the blastopore in *Platydactylus* after Will). The oval embryonic shield lies on the blastoderm; only part of the latter is represented.

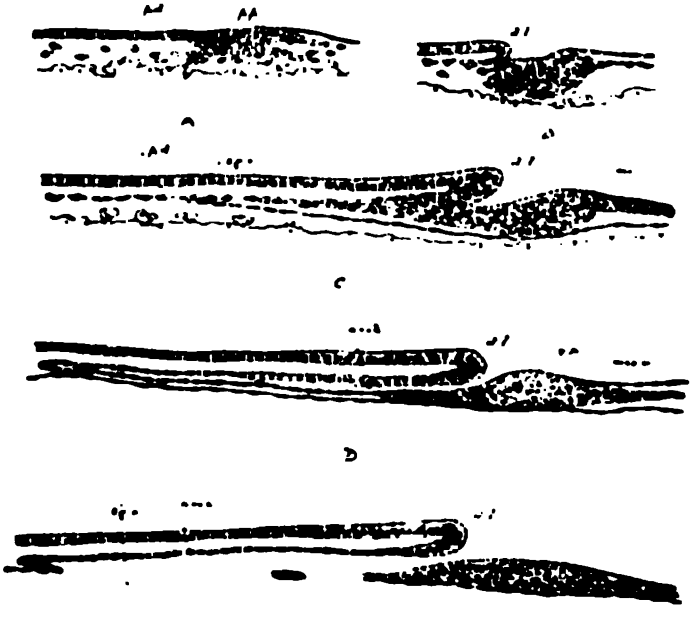

FIG. 25.

Platydactylus (after Will).

Sagittal sections of five stages in the development of the blastopore and archenteron.

A. The upper layer and the paraderm (*p. d.*) are fused in the primitive plate (*p. p.*) at the posterior margin of the embryonic shield; the latter is distinguished by its columnar cells from the flat cells of the surrounding blastoderm.

B, *C*. Formation of the archenteron: *d.l.* dorsal lip, *y. p.* yolk-plug, *mes. e.* mesoderm at ventral lip.

E. Fusion of the archenteric and subgerminal (*s. g. c.*) cavities.

front it consists of a single layer of cubical cells; behind the dorsal lip it is thickened—the primitive plate—and from the thickening there proceeds backwards a narrow tongue of cells between the upper layer and the paraderm.

A transverse section through the blastopore shows the mass of cells of the primitive plate flanked on each side by a projecting blastoporic lip and sending out between the upper layer and the paraderm two lateral sheets of cells (*Fig.* 26, A).

FIG. 26.

A series of four transverse sections through the blastopore and archenteron of *Platydactylus* (after Will).

A. Posterior section showing yolk-plug (*y.p.*) and mesoderm (*mes.*) springing from the lateral lips (*l.l.*). Underneath is the paraderm (*p.d.*).

B. Section in front of the yolk-plug but behind the dorsal lip.

C. Section in front of the dorsal lip; the floor of the archenteron is fused with the paraderm.

D. The floor of the archenteron, together with the underlying paraderm, has come away, and the archenteric and subgerminal cavities are in communication with one another.

The resemblance between these structures—ignoring for the moment the paraderm—and those seen in the Amphibian egg when the blastopore has become circular is sufficiently obvious. The dorsal lip and the lateral lips (there is no ventral lip in the Reptiles) clearly correspond in the two cases; the mass of cells in the primitive plate embraced by these lips is the yolk plug; the cavity of invagination is the archenteron in which

united by a posterior suture behind the primitive streak, which suture he thought might have originated in the same sort of way as the suture which connects the dorsal lip with the yolk-blastopore in an Elasmobranch (*Fig.* 8, A). The paraderm Balfour compared simply to the yolk-cells or yolk of Anamnia, the upper layer to the animal cells or 'ectoderm,' and made the most of the secondary connection between the primitive streak and the paraderm, whereby the necessary continuity of all germ-layers in the lips of the blastopore was effected.

Now to this theory there are two grave objections;

(1) The upper layer contains elements—yolk-plug and floor of archenteron—which must undoubtedly be homologized with some of the yolk-cells of Amphibia;

(2) The edge of the blastoderm in the Amniota does not behave as the lip of the Anamnian blastopore; at it there is no rolling over of cells and no formation of mesoderm.

It may be added that the attempt made by Duval to show that the primitive grove originated at the edge of the blastoderm has completely broken down. The blastopore is from the first moment of its appearance completely within the blastoderm.

Another solution was offered by Rabl (*Fig.* 32, B—D). Recognising the resemblance between the Amniote primitive groove and the *circular* Anamnian blastopore and that the edge of the blastoderm was a new formation, Rabl suggested that the increase of yolk which had taken place in the transition from the Amphibia to the Protamniota had finally ended in the rupture of the body-wall on the ventral side, with the result that in the Amniota the body-wall of the Amphibian embryo with its circular

In other cases the resemblance may be just as striking; in the turtle, *Trionyx*, for example, the yolk-plug projects in the characteristic Amphibian fashion (*Fig.* 28).

The floor of the archenteron now fuses throughout with the paraderm below; and as soon as the fusion is completed perforations begin to appear in the fused layers. They seem to be unable to keep pace with the general growth of the blastoderm, and to become first stretched and then fenestrated. But to whatever causes the perforation may be due, the floor of the archenteron with the underlying paraderm completely disappears, and the archenteron then communicates freely with the sub-germinal cavity (*Fig.* 25, E). The roof of the archenteron is now inserted by its edges into the surrounding paraderm (*Fig.* 27, B). This fusion of the archenteron

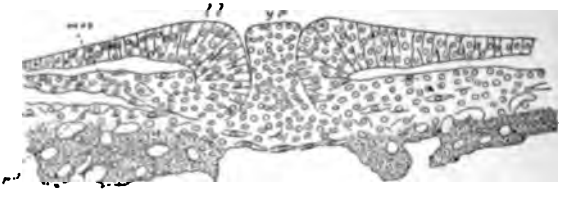

FIG. 28.

Transverse section of the blastopore of *Trionyx* (after Mitsukuri). Lettering as before; the yolk is dotted.

with the sub-germinal cavity is, as we shall have occasion to see more fully later on, quite comparable to the communication of the archenteron with the segmentation cavity in the Gymnophiona; the paraderm must then be regarded as homologous with a part, but only a part, of the yolk-cells in this group.

The median strip of the roof next thickens to form the notochord, and separates from the two lateral portions which then become the mesoderm (*Fig.* 27, B, C, D). These lateral plates pass posteriorly, of course, into the

dorsal lip, where they are perfectly continuous with the sheets of mesoderm produced at the sides of and behind the blastopore. The mesoderm thus exhibits all the relations which it has in the Anamnia.

The lining epithelium of the alimentary canal is derived from the paraderm, which grows in from the sides beneath the mesoderm and notochord towards the middle line. The gut is subsequently folded off from this layer in the ordinary fashion.

The notochord and mesoderm may therefore be said to be laid down in the Reptilia, as we have seen them to be in the Anamnia, at the lips of and during the closure of the blastopore, a closure moreover which is as bilaterally symmetrical here as it is there.

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In *Emys* on the other hand the paraderm takes no part in the formation of any portion of the embryo whatever, but gives rise to the lining of the yolk-sac alone. The roof of the archenteron in this form splits into two sheets, an upper, which again becomes sub-divided into

a median notochord and lateral mesoderm, and a lower which becomes the epithelium of the gut. Such discrepancies recall the similar diversities amongst the Anamnia.

AVES.

The conditions observable in the Birds are very readily derived from and very easily understood in the light of those which obtain in the Reptiles.

The primitive groove is simply a laterally compressed blastopore. In front of the anterior end—the dorsal lip—the notochord is produced (*Fig.* 30, A); to right and left of the notochord are the sheets of mesoderm which, springing from the sides—the lateral lips—of the primitive groove (*Fig.* 29, B), are continued into one another at its posterior end, where there may be an actual ventral lip (*Fig.* 30, B). The archenteric cavity has, however, in most cases disappeared; between the sides of the primitive groove, which, even in those cases in which it is most reduced, exhibit the characteristic structure of blastoporic lips (*Fig.* 29, B), is merely a mass of cells—the representative of the yolk-plug—fused with the paraderm beneath; and the belated 'neurenteric canal' is the sole vestige of the archenteron and the communication which we have seen to be effected between it and the sub-germinal cavity in the Reptiles.

In some cases, however, described by Schauinsland, the archenteric cavity is better developed, and the blastopore quite similar to that of the Reptilia (*Fig.* 30). The primitive streak and groove invariably originate in the upper layer (*Fig.* 29, A), fusion with the paraderm being merely secondary; only after the germ-layers have been formed, can the upper layer be described as ectoderm. The paraderm always gives rise to the alimentary tract; whether it also provides cells for the anterior extension of

the notochord and mesoderm, as in *Lacerta* and some other Reptiles, is perhaps an open question; this] was

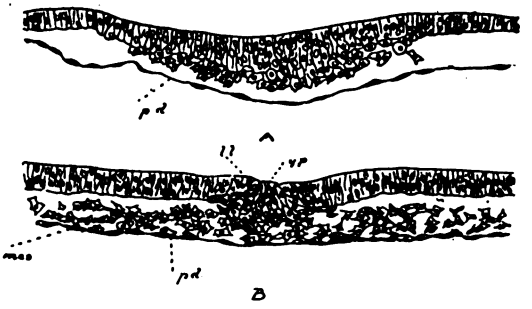

FIG. 29.

Two stages in the formation of the primitive streak and groove in the Chick; transverse sections.

(Original.)

A. 12 hours; proliferation of cells in the upper layer of the blastoderm.

B. 15 hours; formation of lateral lips to the blastopore (primitive groove); a diminutive yolk-plug is seen between them; mesoderm is spreading from the sides of the groove between the upper layer and the paraderm, and the whole is fusing secondarily with the latter.

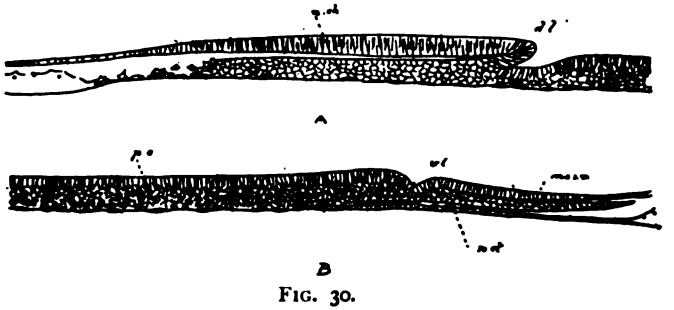

FIG. 30.

Sagittal section of the blastoderm of the Sparrow (after Schauinsland).

A. Anterior half. *B.* Posterior half.

In front of the dorsal lip the notochord has been produced; below it is a slight archenteric cavity.

There is a slight ventral lip with a posterior tongue of mesoderm (*mes.v.*) behind it. The cells of the primitive streak (*p.s.*) have fused with the paraderm (*p.d.*).

stated by Balfour to be the case in the chick, but denied by Kölliker.

MAMMALIA.

The egg of the oviparous Monotremata is megalecithal and meroblastic. At the close of segmentation there are two layers in the blastoderm—an upper layer and a paraderm ; beyond this, nothing of importance is known of the germinal layers.

The egg of the Placental Mammals, on the other hand, is so small as to be almost alecithal ; but there is abundant justification for the view that these Mammals are descended from forms with large-yolked eggs, a justification which is largely based on the mode of formation of the germinal layers, which very closely resembles that observed in the Reptiles and Birds, especially in the former.

The germinal layers, however, only begin to be formed after another—quite peculiar—process has taken place ; and as this process has—in my opinion, quite erroneously—been confounded more than once with 'gastrulation,' or a part of gastrulation, I may perhaps be allowed to state, or rather re-state, the view which I have been led to adopt on this matter.

The fully segmented ovum consists of an outer layer of cells surrounding an inner mass. The outer layer—known as the 'trophoblast,' from the part it subsequently plays in the formation of the placenta—is the homologue of the false amnion, strictly speaking of the ectoderm of the false amnion, of the Sauropsida ; the inner mass contains within itself all the material necessary for the production of the true amnion, the embryo, and the yolk-sac. A cavity next appears separating the inner mass from the outer layer at all points except one, the embryonic pole, and the ovum is now termed a 'blastocyst' ; at the same time the inner mass becomes differentiated into an 'embryonic knob' attached to the trophoblast,

and a layer of flattened cells, the paraderm, which begins to grow round the blastocystic cavity. Segmentation therefore is followed in the Placentalia by the separation of the elements of the trophoblast from those destined to give rise to the embryo and the remainder of its fœtal membranes, and this 'precocious segregation' seems to have occurred phylogenetically during the gradual loss of yolk which the egg of these Mammals has undergone.

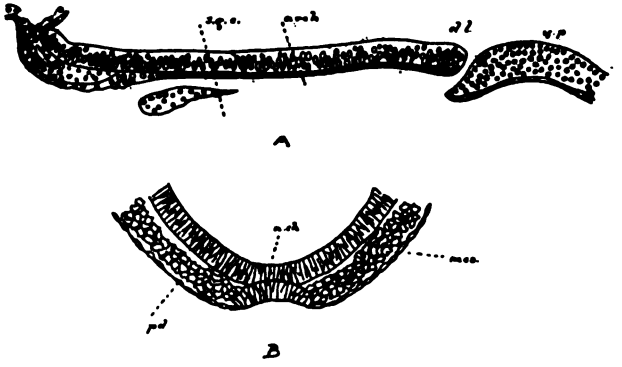

FIG. 31.

Formation of the germ layers in Mammals.

A. Vespertilio (after Van Beneden). Sagittal section showing the archenteron opening to the exterior behind the dorsal lip, and communicating with the subgerminal cavity (*s.g.c.*) below. A small portion of the floor remains.

B. Transverse section of the embryonic area of the mouse. (Original). The roof of the archenteron (which will become the notochord) is inserted by its edges into the paraderm; from its sides spring two lateral sheets of mesoderm.

From a part of the embryonic knob the true amnion is eventually formed, in a manner which varies in different types. The rest of this knob becomes the upper layer of the embryonic area, and is the equivalent of the embryonic shield of the blastoderm in the Sauropsida.

The development of the germinal layers now proceeds. As in the Sauropsida, the necessary material is brought

into position by the bilateral closure of a blastopore and simultaneous formation of an archenteron. These processes originate here, as in the Birds, exclusively in the upper layer, though subsequently a secondary fusion with the paraderm occurs. The blastopore may be laterally compressed—a primitive groove—and the archenteric cavity practically absent or reduced to a very narrow 'neurenteric' or 'chorda-canal'; but in some cases—the best instance is *Vespertilio* (van Beneden) (*Fig.* 31, A)—the blastopore and archenteron are as well developed as in the Reptiles. The archenteron always comes into secondary communication with the subgerminal blastocystic cavity. In front of the dorsal lip the notochord is formed in the ordinary manner from the roof of the archenteron or mass of cells—'Kopffortsatz' of the older authors—which is its representative; from the sides of the blastopore spring the lateral sheets of mesoderm confluent, as in other cases, with one another behind (*Fig.* 31, B). According to many authors, the blastoporic lips—the primitive groove—are the sole seat of formation of notochord and mesoderm, but Heape has described in *Talpa*, and Hubrecht in *Sorex* and *Tarsius*, the formation of an anterior portion of both notochord and mesoderm from the paraderm;[10] and the latter author has further observed the origin of a peripheral ring of mesoderm from the lower layer in the same two genera. This peripheral mesoderm has been recorded by Bonnet in the sheep, where, however, it is denied by Keibel.

The paraderm—growing in from the sides—seems to complete the endoderm in all cases.

[10] Mr. Assheton informs me that this also occurs in the Rabbit.

We can now summarize the principal facts for the Amniota as follows:

(1) A bilateral blastopore is formed within the blastoderm and towards its posterior end.

(2) The blastopore leads into an archenteron, which is also bilateral, its extent being greatest beneath the dorsal lip. The archenteron is developed wholly from the upper layer.

The primitive groove of Birds and Mammals is a laterally compressed blastopore, the 'neurenteric canal' ('chorda-canal'), a rudimentary archenteron.

(3) The floor of the archenteron fuses with the paraderm secondarily, its cavity secondarily with the subgerminal cavity. The roof of the archenteron is then inserted by its edges into the outlying paraderm.

(4) The notochord is formed from the roof of the archenteron in front of the dorsal lip, the mesoderm from the same layer at the lateral lips, the two sheets of mesoderm being continuous behind the blastopore. Additions may be made to the notochord and mesoderm from the paraderm.

(5) The lining of the alimentary canal is usually, though not always, a derivative of the paraderm.

(6) As in the Anamnia, discrepancies are observable in the destinies of sets of cells of similar origin.

It only remains for us to consider the possibility of deriving the Amniote from the Anamnian condition.

THE RELATION OF THE BLASTOPORE OF THE AMNIOTA TO THAT OF THE ANAMNIA.

The first serious attempt to get over the difficulty was made by Balfour. Balfour held that the primitive groove [of the Amniota represented only the dorsal portion of the

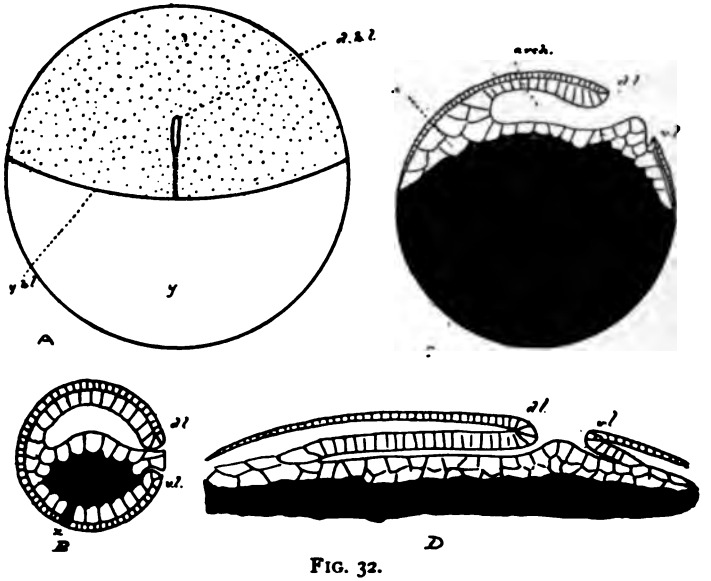

FIG. 32.

A. Diagram to illustrate Balfour's view of the relation of the primitive groove of Amniota (*d.b.l.*) to the edge of the blastoderm or lip of the yolk blastopore (*y.b.l.*). The two are connected by a posterior suture (the two dotted lines); *y.* yolk.

B—D. Diagrams to illustrate Rabl's view of the relation of the Anamnian to the Amniote blastopore (after Keibel).

B. Amphibian egg with closed blastopore and ventrally placed yolk-mass; the latter is bursting through the ventral body wall at *x.*

C. Result of augmentation of the yolk.

D. Protamniote condition reached by still further enlargement of the yolk. The yolk is black in all three figures; *y.c.* yolk cells.

Anamnian blastopore (*Fig.* 32, A, *d. b. l.*), the ventral portion being found in the edge of the blastoderm (*y. b. l.*); the two portions he supposed to have been originally

united by a posterior suture behind the primitive streak, which suture he thought might have originated in the same sort of way as the suture which connects the dorsal lip with the yolk-blastopore in an Elasmobranch (*Fig.* 8, A). The paraderm Balfour compared simply to the yolk-cells or yolk of Anamnia, the upper layer to the animal cells or 'ectoderm,' and made the most of the secondary connection between the primitive streak and the paraderm, whereby the necessary continuity of all germ-layers in the lips of the blastopore was effected.

Now to this theory there are two grave objections;

(1) The upper layer contains elements—yolk-plug and floor of archenteron—which must undoubtedly be homologized with some of the yolk-cells of Amphibia;

(2) The edge of the blastoderm in the Amniota does not behave as the lip of the Anamnian blastopore; at it there is no rolling over of cells and no formation of mesoderm.

It may be added that the attempt made by Duval to show that the primitive grove originated at the edge of the blastoderm has completely broken down. The blastopore is from the first moment of its appearance completely within the blastoderm.

Another solution was offered by Rabl (*Fig.* 32, B—D). Recognising the resemblance between the Amniote primitive groove and the *circular* Anamnian blastopore and that the edge of the blastoderm was a new formation, Rabl suggested that the increase of yolk which had taken place in the transition from the Amphibia to the Protamniota had finally ended in the rupture of the body-wall on the ventral side, with the result that in the Amniota the body-wall of the Amphibian embryo with its circular

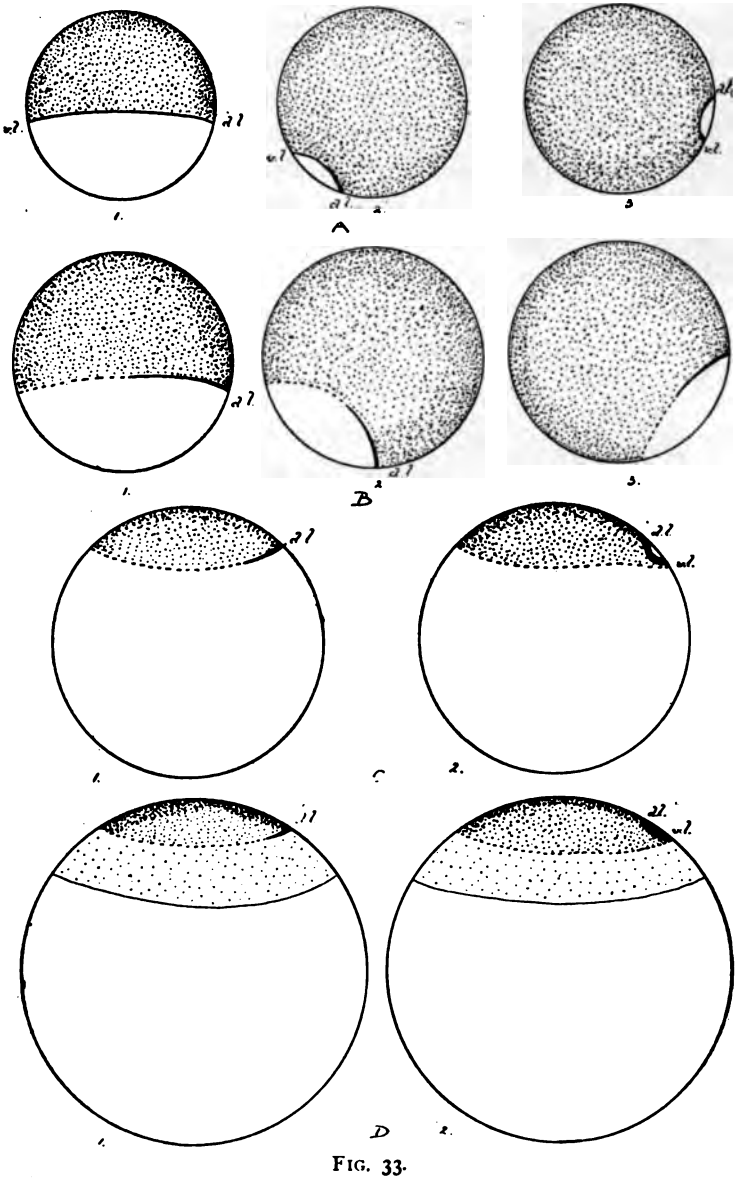

FIG. 33.

Diagrams of the relation of the Amniote blastopore to that of the *Gymnophiona* and of that of the *Gymnophiona* to that of other Anamnia. (Original.)

blastopore is represented by a blastoderm lying on the surface of the yolk and including the primitive streak.

Here again we have to urge a fatal objection. As will occur to everyone, and as Keibel has already pointed out, increase of yolk must take place at the vegetative pole while the egg is still in the ovary, and therefore long before gastrulation can have come to pass and the yolk assumed its ventral position. An increase in the yolk could only delay (as in the fishes) or modify (as in the Gymnophiona) the closure of the blastopore; whereas it is the already closed blastopore with which Rabl begins.

Keibel himself has put forward a theory—which I cannot discuss here—that gastrulation in the Amniota takes place in two phases, the second of which is expressed in the primitive groove; rejecting Rabl's hypothesis he returns to the earlier view of Balfour and Duval. There is still one more suggestion, that brought forward by Ziegler, which must be briefly noticed. Ziegler, indeed, comes near the mark in taking the Gymnophiona as his starting point, though he misses it very widely in

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In all the diagrams the (Anamnian) blastoderm is dotted, and that part of its edge which becomes a blastoporic lip represented by a thick black line.

A. The Frog.

(1) At the end of segmentation.

(2) When the blastopore is circular.

(3) After rotation.

B. Lepidosiren.

(1), (2), and (3), similar stages.

There is no ventral lip.

C. Gymnophiona.

The yolk is increased, the blastoderm smaller, the region of its edge which becomes a blastoporic lip more limited than in *Lepidosiren*; but a ventral lip is formed in (2) by union of the lateral lips.

D. Amniota.

The blastopore is formed as in *Gymnophiona* at the edge of the embryonic shield (the inner darkly dotted area); the blastoderm is enlarged by the addition of a surrounding belt of yolk-cells (outer lightly dotted area).

making the Gymnophionan correspond to the whole of the Amphibian blastopore, and in returning to Rabl's erroneous conception of the ventral augmentation of the yolk.

So far then no theory has been propounded which, in my opinion at any rate, affords an adequate explanation of the facts. And yet the explanation is obvious enough. In the Gymnophiona (*Fig.* 33, C), as we have seen already,

(1) The blastoderm is a circular area of columnar cells resting upon and surrounded by a partially segmented yolk.

(2) At the posterior margin of this blastoderm a dorsal lip is formed and lateral lips quickly follow; the lateral lips then grow back encircling a small area of the yolk behind which they meet and fuse to form a ventral lip to the now circular blastopore. In this process the anterior margin of the blastoderm is wholly unconcerned.

(3) The archenteron which is formed at the blastoporic lip, opens into the segmentation cavity.

(4) Notochord and mesoderm are derived from the ingrowth at the lip of the blastopore, while the endoderm arises from the yolk-cells.

And now let us describe the same stages in the development of a Reptile (*Fig.* 33, D).

(1) The embryonic shield is a circular area of columnar cells resting upon a paraderm and surrounded by a zone of flattened cells.

(2) At the posterior margin of this embryonic shield a dorsal lip is formed and lateral lips quickly follow; the lateral lips then grow back encircling

a small area of the outer zone of cells behind which they meet and fuse to form a (virtual, or in some cases an actual) ventral lip to the now circular blastopore. In this process the anterior margin of the embryonic shield is wholly unconcerned.

(3) The archenteron which is formed at the blastoporic lip opens into the sub-germinal cavity.

(4) Notochord and mesoderm are derived from the ingrowth at the lips of the blastopore, while the endoderm arises from the paraderm.

I venture to think that, considering the difference in the amount of yolk present in the two cases, a closer parallelism could hardly have been expected; *mutatis mutandis* the description of either might be applied to the other type.

It seems clear then that

(1) The embryonic shield of the Amniota is the representative of the blastoderm of the Anamnia.

(2) The marginal zone of the upper layer of the Amniota, together with the paraderm, represents the yolk-cells or nucleated yolk of the Anamnia (*Fig.* 32, D).

In passing from the Gymnophiona—which I suppose are as nearly related as any living group to the ancestors of the Amniota—to these higher forms, we have, therefore, only to suppose that with the enlargement of the vegetative hemisphere of the egg segmentation has become restricted, not to the blastoderm alone (as in the fishes), but to the blastoderm and those circumjacent and subjacent cells which in the Gymnophiona are partially segmented from the yolk. In the most primitive Reptiles the paraderm cells are still crowded with yolk, and still

retain a connection, in the primitive plate, with the cells of the upper layer. In the higher forms—the Birds and Mammals—this similitude of the paraderm to Amphibian yolk-cells is lost, as well as the connection between the two layers in the primitive plate. The upper layer fuses only secondarily with the paraderm when the primitive groove and streak are developed.

There still remains an accessory problem. What is the relation between the Gymnophiona and the rest of the Anamnia? The Gymnophionan blastopore though apparently precisely similar to that of the frog corresponds, as we have seen, only to a part of the latter. It is clear, therefore, that the former must be derived from a blastopore in which the anterior edge of the blastoderm takes no part and which is consequently devoid of a ventral lip. And very fortunately such a blastopore is to be found in a form which not only is less megalecithal than the Gymnophiona, but which may be presumed to stand not far from the direct line of Amphibian descent; I refer, of course, to *Lepidosiren* (*Fig.* 33, B), which may be taken back in its turn, if necessary, to *Petromyzon*. The increase of yolk that has taken place between the Dipnoi and the Gymnophiona has simply resulted in the restriction of the process of germ-layer formation to a perpetually diminishing region of the blastoderm placed symmetrically about the posterior margin or dorsal lip (*Fig.* 33, C). These lateral lips—fusing behind in a ventral lip—have then produced a circular blastopore superficially resembling that of the remaining Amphibia (*Fig.* 33, A). The relation between the two is, of course, just that which Balfour supposed to exist between the primitive groove of the Amniota and the Anamnian blastopore; but it hardly needs, I hope, to be pointed out that the present theory differs *toto coelo* from his; for the representative in the

Amniota of that portion of the edge of the Anamnian blastoderm, which in the Gymnophiona is excluded from participation in the lip of the blastopore, is to be sought not in the edge of the blastoderm but in the margin of the embryonic shield.

It is interesting to notice that in various Anamnia—*Petromyzon* and *Rana*—the same union of the archenteric and segmentation cavities which is observed in Gymnophiona and Amniota also occurs.

Before concluding this discussion I ought to glance at the view which Oscar Hertwig has recently published. Without attempting any more serious solution than is contained in the hint that the clue to the problem must be sought in the Gymnophiona, Hertwig expresses it as his opinion that the separation of the Amniote blastoderm into upper and lower layers represents in a measure a process of gastrulation though the cavity of invagination is entirely absent. To the archenteron of the Amniota, on the other hand, he refuses a complete homology with the similarly named cavity in the lower forms; it is, according to him, merely a 'Mesodermsackchen' involved solely in the production of the middle layer and the notochord. Although it is true that the homology in question is—if destiny as well as origin be taken into consideration—usually, though not always, an incomplete one, still for the purposes of comparison of one and the same stage in development throughout the series it may be regarded as complete; for the upper layer contains in the floor of the invagination cavity and the primitive plate cells which can and must be compared with the cells which line the floor of the archenteron and form the yolk-plug in the Anamnia.

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The broad features of germ-layer development in the whole group of Vertebrata may now be summed up in the following formulæ.

(1) By the germinal layers are to be understood the ectoderm—the source of the epidermis and its derivatives, of the nervous system and the organs of special sense; the endoderm—or lining epithelium of the alimentary tract and its diverticula; and the mesoderm (including the notochord)—the layer which gives rise to the musculature, the skeletal and connective tissues, the vascular system and the urogenital organs.

(2) The material for the ectoderm, notochord and mesoderm, and roof of the archenteron is brought into position during an overgrowth and in-growth of cells which takes place at the lip of the blastopore during the closure of the latter.

(3) This closure is bilaterally symmetrical, taking place more actively at the dorsal lip than at any other point, and leads to the formation of a bilateral archenteric cavity, the extent of which is greatest anteriorly and least posteriorly.

(4) At the lips of the blastopore the ectoderm, the roof of the archenteron, and the notochord and mesoderm are continuous with one another.

(5) The notochord lies medially in front of the dorsal lip; the mesoderm sheets spring from the lateral lips and pass into one another posteriorly behind the ventral lip.

(6) (*a*) The blastoporic lips are formed from the margin of the blastoderm (Anamnia) or of an embryonic shield lying wholly within the blasto-

derm (Amniota). The dorsal lip is always formed at the posterior margin.

(*b*) In all Anamnia, except the Gymnophiona, the ventral lip is developed immediately (microlecithal forms), or after a considerable interval (megalecithal forms), from the anterior margin of the blastoderm.

(*c*) In the Gymnophiona and Amniota the ventral lip is always developed by the fusion of the lateral lips, the anterior margin of the blastoderm (or embryonic shield) taking no part whatever in the process.

(*7*) Considerable discrepancies exist in the manner in which the endoderm is formed in different types. This may arise from the roof only, the floor only, or from both roof and floor of the archenteron, or from the paraderm.

It is these anomalies which lead us naturally to a critical consideration of the morphological value of the germinal layers.

Part II.—The Morphological Significance of the Germinal Layers.

The phylogenetic speculations which have so long dominated our conceptions of development are all directly traceable to a hypothesis first enunciated by Ernst Haeckel, thirty years ago. This hypothesis is the 'Gastraea-Theorie.'

There occurs, according to this theory, in the ontogeny of all multicellular animals, a two-layered form, the Gastrula, produced by invagination or some modification of that process from a one-layered form, the Blastula. The two

layers of the Gastrula are the primary germ-layers, the ectoderm and the endoderm, and are homologous or morphologically similar throughout the series ; the cavity of the Gastrula is the archenteron or primitive gut, and invariably becomes the alimentary canal ; the opening of the cavity is the primitive mouth. With the mesoderm the theory is as such not concerned. Further, this Gastrula is a recapitulation in ontogeny of a phylogenetic form, the Gastraea, the common ancestor of all the Metazoa ; to this primeval form the living Coelenterates are most nearly allied. Before proceeding to examine the credentials of this creed, we may glance briefly at some of its later successors and descendants.

The planula theory of Lankester—put forward almost contemporaneously—closely resembles the hypothesis of Haeckel, differing only from it in substituting for the Gastrula a Planula or Gastrula without a mouth, in which the primary endoderm arises by delamination instead of invagination. The theory was intended to meet the difficulty presented by the variability in the fate of the blastopore.

Passing over a few years we find Balfour[11] returning to a belief in the Gastrula as a form which 'reproduces 'with more or less fidelity a stage in the evolution of the 'Metazoa permanent in the simpler Hydrozoa.' Still later developments of the Gastrula theory, restricted, however, to the Vertebrata, are those propounded by Lwoff[12] and Hubrecht.[13] Lwoff identifies the two primary layers—the animal and vegetative cells—of the vertebrate embryo with the ectoderm and endoderm of the Coelenterate or Gastraea, but part only of the archenteric cavity with the

[11] "Comparative Embryology," vol. 2, ch. 13, 1881.

[12] *Bull. soc. imp. nat. Moscou*, n.s., vol. 8, pp. 57, 160, 1894.

[13] "Furchung und Keimblattbildung bei Tarsius spectrum," *Verh. k. Akad. Wetensch. Amsterdam*, (2), vol. 8, no. 6, 1902.

digestive tract of these. Two distinct processes, according to this author, have hitherto been confused under the head of gastrulation. One—which is alone represented in Invertebrate forms—is an invagination of the primary endoderm into the segmentation cavity; the other—peculiar to the Vertebrates—is the overgrowth and ingrowth of a 'dorsal plate' of cells (the roof of the archenteron) produced entirely from the primary ectoderm and taking place most actively at the dorsal lip of the blastopore. The ectodermal dorsal plate gives rise to notochord and mesoderm, while the alimentary tract is derived entirely from the invaginated endoderm cells. It must be pointed out that, quite apart from the difficulties of the Gastrula theory, of which it is of course a modification, Lwoff's hypothesis is very seriously contradicted by the fact that the roof of the archenteron—his 'dorsal plate' of 'ectoderm'—in many cases either contributes to or forms the whole of the digestive canal. To argue, as Lwoff actually does, that in these cases primary endoderm cells have found their way into the roof of the archenteron is, it is superfluous to point out, destructive of the universality of the 'ectodermal' origin of the latter, for which, as a matter of fact, a good deal might be said.

Hubrecht's theory resembles that of Lwoff in attributing to the 'dorsal' or 'notochordal' plate the sole function of notochord and mesoderm formation; but differs from it in seeing in the segmentation cavity, with which, as this embryologist urges, the archenteron in all cases becomes confluent, the primitive Coelenterate gut, the archenteron itself being then regarded simply as an ectodermal stomodæum.

Now underlying the Gastrula theory and its modifications there are two fundamental assumptions, assumptions which must be vindicated if the theory is to be maintained.

In the first place, it is postulated that the primary layers of the Gastrula are completely homologous, that is, homophyletic or homogenous, throughout the Metazoan series; and in the second, that they bear certain constant and invariable relations to the organization of the adult on the one hand, and to that of the reputed ancestral form on the other. Haeckel recognises this perfectly, 'Der 'wesentliche Inhalt dieser Gastraea-Theorie,' he says 'beruht auf der Annahme einer wahren Homologie (ohne 'welchen die Gastraea-Theorie nicht haltbar ist) der primi-'tiven Darmanlage und der beiden primären Keimblätter 'bei allen Thieren mit Ausnahme der Protozoen. [14]

Further, since, as is held, it is phylogeny which is repeated in and ultimately determines the form which ontogeny takes, any doubt which exists as to the homology of an organ in the fully developed form may be completely set at rest by a reference to the germinal layer in which it arises, the homologies of the germ-layers in their turn being determined by reference to their mutual anatomical relations or, when possible, their mode of origin in the segmenting egg. It is origin then, and not destiny, if we push these theories to their logical conclusion, which is the final criterion of the homologies, whether of the organs of the adult or of the layers of the germ.

The first question accordingly which we have to answer is, are the so-called primary layers always homologous? Let us consider this in the light of the facts which vertebrate development has taught us.

At the close of segmentation the microlecithal vertebrate ovum consists of an animal hemisphere of small cells and a vegetative hemisphere of large cells, identified by Haeckel with the primary ectoderm and endoderm respectively. In the megalecithal eggs segmentation is

[14] *Jenaische Zeitschr.*, vol. 8, p. 10, 13, 1874.

confined to a blastoderm, but in this we distinguish again an upper and a lower layer of cells; as far as their relative positions are concerned, the upper layer corresponds very closely with the animal hemisphere; the lower layer, together with the yolk, to the vegetative hemisphere in the former case. There thus exists a very fairly complete morphological similarity or homology between the primary germinal layers throughout (we except, of course, the Placental Mammals, in which any attempt to identify the outer layer and inner mass with the primary germ layers has long ago been abandoned), though it must be borne in mind that this rests merely on the structure of the fully segmented egg, not on anything that we know of the origin of these layers during segmentation.

To the second question, however, is the relation which these primary layers bear to the parts of the embryo in the next stage constant and invariable, we can most certainly not return an affirmative answer; for while in the Anamnia the floor of the archenteron and the yolk-plug are derivatives of the primary 'endoderm,' in the Amniota they are of purely 'ectodermal' origin; nor is it even possible to say that the cell-layer which forms the archenteric roof is homologous throughout, its 'ectodermal' and 'endodermal' origin having been equally maintained.

If we prefer to compare the primary layers not with the structures visible in the next stage, but directly with the organization of the adult, our difficulties are just as great. For now the alimentary tract of Teleostei (and Elasmobranchii?) is ectodermal, that of *Ceratodus*, Gymnophiona, and Amniota 'endodermal,' that of the Frog and *Lepidosiren* of mixed origin. It is abundantly clear that each of the primary germinal layers, as defined by Haeckel, though constant in its structural relations throughout the series, may be, and often is in its destiny, exceedingly diverse.

But, it will no doubt at once be said, in the Vertebrates the Gastrula must be looked for, not at the end of segmentation, but in that stage in development when the blastopore is already closing. Though this is neither Haeckel's two-layered Gastrula (except in the case of the 'Disco-Gastrula' of the Teleostei), nor the Planula of Lankester, we will nevertheless examine the hypothesis in its new form. In this stage the Vertebrate embryo possesses a blastopore leading into an archenteron, the archenteron has a floor and a roof, and from the lips of the blastopore the notochord and mesoderm have been produced between the archenteron and the ectoderm. There is a very close similarity between all embryos at this stage, and we may—if we disregard for the moment the differences in their mode of origin—fairly describe their parts as homologous. But do these homologous parts exhibit that absolute constancy in their relation to adult structures which the theory demands? By no means; the ectoderm truly has the same fate in all cases, except when, as in the skin glands of Amphibia, it gives rise to muscles, or, as in the thymus of Elasmobranchs, to blood-corpuscles, and so has the mesoderm, except when it gives rise to enamel; but the roof and floor of the archenteron vary widely in their subsequent behaviour. The roof may form notochord only, or notochord and mesoderm only, or both these, and either the roof or the whole of the definitive gut. The floor may form a part or the whole of the lining epithelium of the digestive tract, or be excluded from it altogether. It will be urged, of course, that the archenteric cavity nevertheless becomes the alimentary canal; but here the Amniota involve us in great difficulties; so little a part does the archenteron play in the formation of the lumen of the gut that Hertwig has felt constrained to call it a 'Mesodermsäck-

chen,' and Hubrecht a 'Stomodaeum,' looking for the primitive Coelenterate gut in the cavity of the blastocoel. But, perhaps, it will be maintained, this is a mere caenogenetic adaptation, and the embryo does possess in this stage to all intents and purposes a primitive gut opening by a blastopore, an ontogenetic type of frequent, though not—as witness the Planula theory—of universal occurrence. The desired homologies are now sustained only by explaining away as caenogenetic every inconvenient fact, which is suicidal; by refusing to define a cavity by the nature of its walls, which is absurd; and by ignoring dissimilarities in origin, though origin is in every other case held to be the sole ultimate test of community of descent, which is inconsistent. Such reasoning must surely be the very last refuge of the destitute phylogenist.

In fine, if we establish homologies, as we are perfectly entitled to do, on the basis of the structure which the egg presents at the end of segmentation these homologies are without constant reference to those resemblances which appear in the next stage during the closure of the blastopore; and these, in their turn, bear no necessary and definite relation to the structure of the adult forms. Either origin or destiny may be taken as decisive of homology; but origin and destiny do not of necessity coincide. The appeal to destiny must either rob the supposed recapitulatory stage of all independent significance or involve us in a vicious circle, while any attempt to make origin the ultimate criterion will inevitably end in the subversion of that homology which we are bound to predicate of the adult organs through and through. The Vertebrata then afford no support to the Gastræa or to any other theory which attaches a morphological, phylogenetic, or recapitulatory significance, a value for the determination of adult homologies, to the germinal layers.

We have now to consider whether the embryology of other groups promises any better prospect of success.

The two-layered—Gastrula or Planula—stage is, of course, of widespread occurrence in all the Invertebrate groups. Without pausing at present to enquire into the origin of these two primary layers, let us merely follow out their fate in a few cases, and in particular the fate of the endoderm. Frequently, it is true, the endoderm gives rise quite simply to some part of the alimentary canal; but it has always been a source of great difficulty that the region of this canal which arises from the endoderm is so exceedingly variable in extent. While it may and generally does happen that the major part is endodermal in origin, there are several cases amongst the Arthropods in which only a very small midgut is derived from this source, the remainder of the digestive tract being stomodaeal and proctodaeal. Apparently then, unless development misleads us, 'there are,' to quote Balfour, 'instances in which a very large portion of the alimentary canal is phylogenetically an epiblastic structure.'[15] Balfour felt this so strongly that he suggested that there was a border land between the epiblast and the hypoblast which might be developed indifferently from either. Worse, however, is to follow. The endodermal midgut, already reduced in these cases almost to a rudiment, may in others entirely disappear.

In the Cephalopoda, the alimentary canal, according to Watasé,[16] (though this is denied by Korschelt,[17]) is formed entirely of the stomodaeal and proctodaeal invaginations, the yolk-sac epithelium—which is unani-

[15] "Comparative Embryology," vol. 2, ch. 13.

[16] *Stud. Biol. Lab. Johns Hopkins Univ.*, vol. 4, p. 163—183, 1887—90.

[17] "Festschrift für Leuckart," Leipzig, 1892, p. 347—373.

mously identified with the primary endoderm—being wholly excluded from it. Watasé, indeed, goes so far as to say on the strength of this result that the digestive systems of a squid and a snail are not homologous. Earlier observers, it may be added, had also failed to trace the endoderm into the gut. And this is not a solitary example. In 1884 Witlaczil[18] described the origin of the alimentary canal in Aphis from stomodaeum and proctodaeum alone; Voeltzkow[19] has maintained the same of *Musca*; Lécaillon[20] of various Coleoptera; while Heymons,[21] it is interesting to note, though denying to the yolk-cells any share in the development of the gut in Orthoptera, has shown that in *Lepidura* and *Campodea* the digestive cavity arises in the fashion required by theory from the primary endoderm.

Physiologically it is not difficult to understand these anomalies; they find a parallel, for example, amongst the Vertebrates in the behaviour of the yolk-nuclei of Elasmobranchs and Teleostei, structures set aside for the elaboration of the yolk but playing no part in the production of the embryo. Morphologically, however, they offer obvious and insuperable difficulties to the ordinary theories of gastrulation.

Nor is it merely in the development of the digestive cavity that this want of harmony between origin and destiny makes itself so painfully felt. Irregularities of this kind are exhibited by all the germ-layers, and often in the most startling and irrefutable manner. For

[18] *Zeitschr. wiss. Zool.*, vol. 40, p. 559–696, 1884.

[19] *Arb. Zool. Inst. Würzburg*, vol. 9, p. 1–43, 1889.

[20] "Rech. sur l'œuf et devel. embryon. de quelques chrysomélides." *Thèses Fac. sci. Paris*, ser. A, no 299, 1898; abstr., *Zool. Centralblatt*, vol. 5, p. 813–6, 1898.

[21] *Sitzungsb. Gesell. Naturf. Freunde Berlin*, 1897, p. 111–1233 *Sitzungsb. Akad. Wiss. Berlin*, 1894, pt. 1, p. 23–27.

fortunately it is possible, in the 'cell-lineages' of many Molluscs, Annelids, and Turbellarians, by tracing back an organ or layer to a single cell to establish its origin beyond cavil or question. It will be sufficient for the purposes of this argument to cite the most striking discrepancies.

In the three groups just mentioned the segmentation of the egg proceeds upon a very regular so-called 'spiral' plan (*Fig.* 34). The first two divisions—meridional, and at right angles to one another—produce four cells

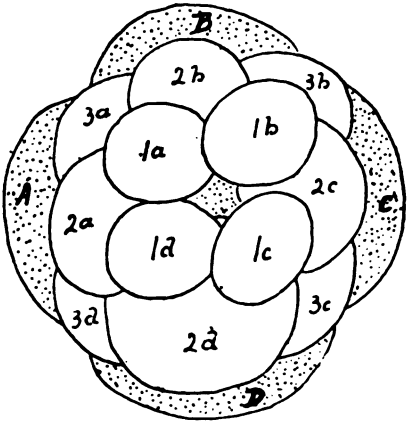

FIG. 34.

Diagram of the formation of the first (1*a*—1*d*), second (2*a*—2*d*), and third (3*a*—3*d*) quartettes of micromeres from the macromeres (*A*–*D*) in the egg of a Turbellarian, Mollusc, or Annelid.

lying, roughly speaking, in one plane. Of these four cells, one is left, one is anterior, one right, and one posterior; they are known as the macromeres, and termed respectively A, B, C, and D. Each macromere now buds off towards the animal pole, and in a right-handed direction a small cell or micromere; the four constitute the first quartette, and are termed (following Conklin's notation) 1*a*, 1*b*, 1*c*, and 1*d*. A second quartette (2*a*, etc.) is given off left-

handedly, a third quartette (3*a*, etc.) right-handedly, and a fourth (4*a*, etc.) left-handedly again. The micromeres themselves obey the same law of alternating direction of cleavage in successive divisions. These four quartettes and the residual macromeres contain the material for the germinal layers. In one case (*Tethys*)[22] a fifth quartette of micromeres is produced.

Let us now follow, first, the fate of these cells in different cases.

The first quartette becomes the ectoderm of the pre-oral region, including the apical organ, when present, the cerebral ganglia, and the whole or a part of the prototroch.

In the second quartette the posterior blastomere, 2*d*, is often very large, and known as the first somatoblast; it gives rise to the ventral plate of ectoderm. The cells in the remaining quadrants (2*a*, 2*b*, 2*c*) usually also give rise to ectoderm, forming sometimes the stomodaeum or oesophagus (*Capitella*),[23] sometimes contributing cells to the prototroch. There are, however, many cases in which mesoderm also arises from this group of cells; in *Crepidula*[24] derivatives of 2*a*, 2*b*, 2*c* produce larval mesoblast, in *Unio*[25] a descendant of 2*a*, in *Arricia*[26] possibly 2*c* and 2*d*; in *Aplysia depilans* (according to Georgevitch[27]) 2*c* and 2*d* are the primary mesoblasts, and in *Dreissensia*[28] mesenchyme is derived from the descendants of all four cells; while in the Turbellaria, according to Hallez[29] and Lang[30], the blastomeres of this quartette are

[22] Viguier, C., *Arch. Zool. Exp.* ser. 3, vol. 6, p. 37–58, 1898.
[23] Eisig, H., *Mitt. Zool. Stat. Neapel*, vol. 13, p. 1–267, 1899.
[24] Conklin, E. G., *Journ. Morph.* vol. 13, p. 1–204, 1897.
[25] Lillie, F., *Journ. Morph.*, vol. 10, p. 1–84, 1895.
[26] Wilson, E. B., *Biol. Lect. Woods Holl.*, 1898, p. 21–42.
[27] *Anat. Anz.*, vol. 18, p. 145–174, 1900.
[28] Meisenheimer, J., *Zeitschr. wiss. Zool.*, vol. 69, p. 1–137, 1901.
[29] "Contributions à l'histoire naturelle des Turbellariés." Lille, 1879.
[30] "Die Polycladen des Golfes von Neapel. Leipzig, 1884.

devoted to the production of mesoderm alone, though Wilson[81] has stated that in Leptoplana some ectoderm is formed from them as well.

Similarly, while the cells of the third quartette are usually devoted to ectoderm formation—ectoderm which may surround the blastopore or provide the lining for the stomodaeum or proctodaeum—there are many cases in which a part, for example 3*c* 1, 3*d* 1 in *Capitella*,[82] 3*b* 1, 3*c* 1 in *Physa*,[83] derivatives of 3*a*, 3*d*, 3*c* in *Podarke*[84] and of all four quadrants in *Dreissensia*,[85] 3*d* in *Teredo*[86] and *Cyclas*[87], or the whole, as in the Turbellaria—though again this is denied by Wilson—of these blastomeres contributes to the larval or permanent mesoderm.

In the fourth quartette the posterior cell 4*d* is as a rule set aside for the exclusive development of the mesoblast bands; but in many cases this blastomere contains endodermal elements as well (*Nereis*[88], *Crepidula*, *Aplysia*, *Podarke*, *Dreissensia*); while in the Turbellaria, and apparently in *Teredo*, it is purely endodermal. In *Capitella*, on the other hand, it gives rise not to the mesoblast bands, but to the larval mesoderm, together with some ectoderm, and in *Tethys* it is stated by Viguier to be restricted with the other cells of the same quartette to the ectoderm.

The remaining cells of this quartette (4*a*, 4*b*, 4*c*) with the residual macromeres (A, B, C, and D) constitute the endoderm. Here there are fewer exceptions although, as

[81] *Loc. cit.*

[82] Eisig, H., *Loc. cit.*

[83] Wierzejski, A., *Biol. Centralbl.*, vol. 17, p. 388—394, 1897.

[84] Treadwell, A. L., *Journ. Morph.*, vol. 17, p. 399—486, 1901.

[85] Meisenheimer. *Loc. cit.*

[86] Hatschek, B., *Arb. Zool. Inst. Wien*, vol. 3, p. 1—42, 1881.

[87] Stauffacher, H., *Jenaische Zeitschr.*, vol. 28, p. 196—240, 1894. Wilson, E. B., *Journ. Morph.*, vol. 6, p. 361—480, 1892.

just mentioned, *4a*, *4b*, *4c* in *Tethys* are ectodermal and in *Cyclas* and *Teredo* the macromeres in these three quadrants as well; (in the two latter genera the first division is so unequal, as also the following division of the larger posterior cell, that A, B, and C were termed by Stauffacher and Hatschek micromeres in contrast to the larger macromere D). In *Clepsine* no endoderm is derived from D[39].

There is clearly then no necessary regularity in the subsequent behaviour of cells which in origin are identical; and if now conversely, we trace back identical organs to their origins precisely the same lack of agreement will become manifest.

The prototroch, for example, though always ectodermal, is not limited to a particular set of cells: for while it is true that certain cells in the first quartette—derivatives of *1a. 2.*—*1d.2*—always take part in its formation, it frequently happens that these are reinforced by secondary trochoblasts derived from the second quartette (*Crepidula*, *Trochus*,[40] *Amphitrite*,[41] etc.). The stomodaeum again, usually formed from the second, may (*Arenicola*)[42] arise from the third, or sometimes (*Capitella* and *Ischnochiton*)[43] from both second and third quartettes. Nor does the ectoderm itself even escape these variations; ordinarily derived from the first three quartettes of micromeres its formation may (Turbellaria, though not according to Wilson) be limited to the first quartette, or on the other hand extended to the fourth (*Tethys*) and even to some of the residual macromeres (*Cyclas* and *Teredo*)

[39] Whitman, C. O., *Quart. Journ. Micr. Sci.*, vol. 18, p. 215–315, 1878.
[40] Robert, A., *Arch. Zool. Exp.*, ser. 3, vol. 10, p. 270–513, 1902.
[41] Mead, *Journ. Morph.*, vol. 13, p. 227–326, 1897.
[42] Child, C. M., *Arch. Entwickl.*, vol. 9, p. 587–709, 1900.
[43] Heath, H., *Zool. Jahrb.* (*Anat.*), vol. 12, p. 567–656, 1899.

In the origin of the mesoderm the anomalies are even more serious. In the majority of cases it is, truly, derived from *4d*, though even then, it must be remembered, this cell may contain endodermal elements; but it may and frequently does happen that the mesoderm receives additional elements from other sources. In *Crepidula* descendants of *2a*, *2b*, *2c*, in *Unio* of *2a*, in *Aricia* either of *2c*, *2d*, or of *3c*, *3d*, in *Physa* of *3b*, *3c* produce larval mesoderm; in *Dreissensia* and *Limax*[44] mesenchyme arises from both second and third quartettes; in *Capitella* *3c.* 1 and *3d.* 1 are the pole cells of the mesoblast bands, *4d* producing merely larval mesoderm. Long ago Lankester[45] showed that in *Pisidium* the ectoderm of the Gastrula could give off mesenchyme cells, and now Meisenheimer and Tönniges[46] have demonstrated that in *Dreissensia*, *Limax*, and *Paludina*, while the mesoblast bands originate from *4d* in the typical manner, the material for the heart, pericardium, kidneys and gonads is derived from the ectoderm later on. In *Aplysia*, on the authority of Georgevitch, though not of Carazzi,[47] *2c* and *2d* are the primary mesoblasts, in *Tethys* *5d* instead of *4d*, while in the Turbellarians (*pace* Wilson) the second and third quartettes alone produce mesoderm and mesoderm alone.

Lastly, the endoderm is in *Cyclas* and *Teredo* confined to quadrant D, while in *Tethys* it is only formed after a fourth quartette of ectomeres has been given off.

Here, then, in a series of forms in which we can apply the test of origin in the most decisive and unequivocal manner to questions of homology, that test breaks in pieces in our hands; here, as in the Vertebrates, origin

[44] Meisenheimer, J., *Zeitschr. wiss. Zool.*, vol. 62, p. 415—468, 1897.

[45] *Phil. Trans.*, vol. 165, p. 1—48, 1875.

[46] *Zeitschr. wiss. Zool.*, vol. 61, p. 541—605, 1896.

[47] Georgevitch (*l.c.*) describes *A. depilans*; Carazzi (*Anat. Anz.*, vol. 17, p. 77—102, 1900), *A. limacina*.

and destiny do not in any cell, or set of cells, invariably and necessarily coincide; homologies founded on the former may be the diametrical opposite of comparisons based upon the latter, and in this dilemma it is to the latter only that we can continue to adhere without reducing comparative anatomy to an absurdity.

The facts of descriptive embryology might well be deemed sufficient of themselves to warrant us in thus finally relinquishing any hope of retaining the morphological or phylogenetic significance which for so long has been attached to the germinal layers. Corroborative evidence is, however, forthcoming from other quarters, from the phenomena of budding and segmentation, from pathology, and from the results of experimental embryology. Let us briefly consider these.

We have seen that in the normal development of the fertilized ovum there comes a moment, at any rate in some forms, when it is possible to detect three layers or sets of cells which, however much they may vary in their origin, bear constant relations to certain definite parts of the adult organization by reference to which they may be defined. Even this constancy is, however, not absolute; muscles, for example those of the skin-glands in Amphibia, may be derived from the ectoderm, and enamel is said occasionally to arise from mesodermal tissues. In the mode of reproduction termed budding this inconstancy may be much more marked.

The difficulties presented by the anomalous behaviour of the germ-layers, or rather of their representatives, in the budding of various Ascidians, is well known to zoologists. The young bud is a two-layered sac, the outer layer being formed in all cases of ectoderm; the inner layer, on the other hand, may be a diverticulum of the

atrium of the parent (Botryllidae), in which case it is ectodermal, or may arise from the dissepiment of the stolon (*Clavellina*, *Didemnum*), in which case it is a derivative of the epicardium, that is to say of an organ of endodermal origin. Now, in spite of this diversity, the behaviour of the inner layer is by the concurrent testimony of Seeliger, Oka, Pizon, and Hjort[48], essentially the same in all cases; it gives rise, in fact, to the pharynx and alimentary canal, to the atrial cavity, and, according to Hjort, also to the hypophysis and nervous ganglion of the bud, though a different mode of origin of the last-mentioned is described by the other observers. While therefore in the first case an organ, the alimentary tract, which in the embryo is derived from the endoderm, is in the bud of ectodermal origin; in the latter organs, the atrium and nervous system, which are ectodermal in the larva are developed in the bud from the other layer.

The relation of gemmation to the germ-layers in the Ectoproctous Polyzoa is just as great a source of perplexity. After segmentation, a two-layered stage is reached, either by unipolar immigration or a process which is reducible to this. The inner layer so formed—comparable as far as its origin goes with the primary endoderm of other forms—never takes any part in the development of the adult alimentary canal; although in those few cases amongst the Gymnolaemata in which the larva possesses a gut, this organ apparently arises from these cells, the remainder becoming mesoderm. Where the larva has no gut most of this inner mass disintegrates and is ultimately found, together with the degenerating larval organs, in the 'brown body;' a small

[48] Seeliger, O., *Sitzungsb. Akad. Wiss. Wien*, vol. 85, pt. 1, p. 361—413, 1882.
Oka, A., *Zeitschr. wiss. Zool.*, vol. 54, p. 521—547, 1892.
Pizon, M. A., *Ann. Sci. Nat.* (*Zool.*), (7), vol. 14, p. 1—386, 1893.
Hjort, J., *Anat. Anz.*, vol. 10, p. 215—229, 1895.

portion is, however, reserved for the mesoderm of the adult. In the Phylactolaemata, on the other hand, the whole of this layer becomes mesoderm, and the cavity which it circumscribes is shown by its subsequent behaviour to be coelom. The larval organs, when present, bear no relation to the corresponding organs of the adult, but degenerate wholly and pass into the brown body. The organs of the first polypide are formed from a bud which is produced by an invagination of ectoderm covered by a layer of mesoderm; from the ectodermal layer are developed not only the epidermis with the nervous system but the whole of the digestive tract as well."

It is obviously exceedingly hard to reconcile such facts with ordinary germ-layer theories; though an attempt to save the situation has been made by showing that the polypide bud arises (in the Phylactolaemata only) near the original pole of immigration of the inner mass, and that there is a complete gradation from those types in which the inner mass gives rise to the larval gut and to the mesoderm through those in which the endodermal portion degenerates in an early stage, to those in which there is never at any time any endodermal portion at all.

The behaviour of the layers during the regeneration of lost parts is as irregular as it is in budding. This has been particularly shown to be the case in the regeneration of the head and tail in various Annelids. The accounts given by different observers, however, of the changes that take place do not completely accord with one another.

According to Rievel[**] (*Ophryotrocha*, *Nais*, *Allolobophora*, *Lumbricus*) a mass of 'granulation tissue' of meso-

[**] Korschelt, E. und K. Heider, "Lehrbuch der vergl. Entwick. d. wirbellosen Thiere." Jena, 1893.

[**] Rievel, H., *Zeits. kr. wiss. Zool.*, vol. 62, p. 289-341, 1897.

dermal origin is formed over the cut surface. This becomes covered by an overgrowing layer of epidermis, and subsequently perforated by the forward (or backward in the case of the regeneration of the tail) growth of the blind end of the gut; after the fusion of this with the epidermis a new opening—mouth or anus—is effected. It will be noticed that in this case the original stomodaeum and proctodaeum are replaced by endodermal epithelia. Haase's[51] description of the processes that occur in Tubifex is very similar. Michel[52], on the other hand, states for a very large number of Oligochaeta and Polychaeta—and von Wagner[53] (*Lumbricinus*) and Hepke[54] (*Vais*) are at one with him in this matter—that the cap of reparation tissue which developes over the wound is due largely if not entirely to an active proliferation of the epidermis. All three observers agree in deriving not only the new nerve-cord but also the new mesoderm—including the 'ectodermal' nephridia—from this mass of cells; but differ in their description of the regeneration of the gut. According to Michel the intestinal aperture made by the cut never closes, but persists as the anus. Von Wagner also derives the lining of the regenerated portions from that of the old alimentary tract; the gut closes first and then grows through the reparation tissue to fuse with the epidermis and open to the exterior; there is only a very slight invagination at the aperture. Hepke, on the contrary, describes the forward (or backward) growth of a solid cord of cells derived from the epidermal reparation tissue which, acquiring a lumen, opens into the old gut at one end and to the exterior at the other.

[51] Haase, H., *Zeitschr. wiss. Zool.*, vol. 65, p. 211-256, 1899.

[52] *Bull. Sci. de la France et de la Belgique*, vol. 31, p. 245—401, 1898.

[53] *Zool. Jahrb.* (*Anat.*), vol. 13, p. 603-682, 1900.

[54] *Zeitschr. wiss. Zool.*, vol. 63, p. 263-291, 1898.

By the testimony, therefore, of all these authors except Hepke, the original stomodaeum—which includes the pharynx—and proctodaeum are replaced in the regenerated parts by epithelia of ectodermal origin; by Hepke's account these epithelia are ectodermal but replace (apparently) not only the stomodaeum and proctodaeum, but originally endodermal portions of the digestive tract as well. In either case it seems we are involved in a contradiction. With regard to the new formation of the mesoderm there is more agreement; it is derived from the epidermis, a fact which, once more, is out of harmony with its embryonic development.

It should be added that Dendy[55] has shown that in *Antedon* the visceral mass can be regenerated partly from the mesoderm, partly from the ectoderm of the calyx; the new alimentary canal arises from the latter.

In this connection it may be mentioned that cases occur in which tumours resembling one another in every respect may, apparently, have been derived in one instance from the ectoderm, in another from the mesoderm, or even the endoderm. The most remarkable instance of this is, perhaps, that quoted by Hansemann[56] where a malignant growth of the epithelium of the gall-bladder had assumed the structure of an epidermal cancroid. Many pathologists have, therefore, given up the attempt to classify the abnormal growths by reference to their developmental origin.

Last of all it remains for us to discuss the bearing on our question of a very important series of facts which the experimental embryology of recent years has made known to us

[55] *Stud. Biol. Lab. Owens College*, vol. 1, p. 299–312, 1886.

[56] "Die mikroskopische Diagnose der bösartigen Geschwülste," p. 23, Berlin, 1897.

The experiments which most immediately concern us are those instituted by Oscar Hertwig, Driesch, and other embryologists on the segmentation of eggs submitted to pressure, and on the behaviour of isolated blastomeres; experiments which in the first instance arose out of a critical enquiry into the pretensions of the Roux-Weismann hypothesis of preformation. This hypothesis, as it is hardly necessary to point out, is a modern resuscitation of the famous theory of evolution which was destroyed by Wolff more than a hundred years ago, and postulates a necessary predetermination of definite parts of the fertilized ovum, or of its nucleus, for the production of particular organs of the embryo. Segmentation in this case is a qualitative process; all that it has to do is to separate the already different parts, and so make manifest that structure which was invisibly present before; it is, as Roux phrases it, a 'Mosaikarbeit.'

Now at bottom the theories which have been criticised above are also attempts to throw back the structures of the adult into definite predestined parts, not necessarily of the unsegmented, but at least of the segmented ovum; they are in fact essentially preformationist,[56] and, moreover, the logical outcome of that doctrine. For, if development is strictly a process of 'self-differentiation' proceeding wholly from causes resident in each part and independent both of other 'internal' conditions—such as mutual position—and of 'external factors'—such as the physical and chemical composition of the environment—then the potentiality of each part is predetermined and limited, similarly constituted ova will develope through similar stages into related forms, while any dissimilarity occurring at any stage will lead to the production of organs that are not homologous and forms that diverge, and, since the prime cause of differentiation—the structure

[56] This has also been pointed out by Braem (*l.c.* p. 435).

of the fertilized ovum—is itself a heritage from a long line of ancestors, each individual will of necessity repeat in its ontogeny the history of its descent. If, on the other hand, there is no necessary constancy, even within the limits of a single species, in the fate of cells of identical origin, such a regularity need certainly not be claimed for an entire class.

The question is thus narrowed down to a perfectly plain issue, which may be brought to the test of experiment; with the preformationist hypothesis germ layer theories must stand or fall.

We owe the pressure experiments, to which allusion has been made, to Driesch[87] and Hertwig.[88] The former showed in *Echinus*, the latter in the Frog, that by this means the nuclear spindles could be displaced, and the disarranged nuclei made to pass into other than their normal blastomeres, without affecting the normality of subsequent development. In Hertwig's experiments, it is interesting to observe, nuclei pass into vegetative which are ordinarily allotted to animal cells, and *vice versa*. Wilson's[89] experiments on *Nereis* are of a like nature. By pressure the egg is made to segment into a flat plate of eight cells, all of which, when the pressure is released, behave as macromeres. Eventually cells are found in the lining of the gut, which ought to have contributed to the ectoderm of the first quartette.

The results of the experiments on the development of isolated blastomeres fall into a regular series. At one end of this stand the Vertebrates and Amphioxus, the Ascidians[89a] and the Coelenterates; here an isolated blastomere

[87] *Zeitschr. wiss. Zool.*, vol. 55, p. 1–62, 1893.

[88] *Arch. mikr. Anat.*, vol. 42, p. 662–704, 1893.

[89] *Arch. Entwickelungsmech.*, vol. 3, p. 19–26, 1896.

[89a] Driesch, H., *Arch. Entwickelungsmech.*, vol. 1, p. 398–411, 1895. E. G. Conklin has, however, recently shown that in *Cynthia* there is a precocious localisation of definite organ-forming substances in the egg (*Journ. Exp. Zool.*, vol. 2, p. 145–221, 1905).

behaves from the first as an entire ovum, segmenting normally and producing a whole embryo. It may be remarked at this point that Hertwig[60] failed to obtain the half-embryos of the frog which Roux produced by killing one of the first two blastomeres; that Schulze[61] has made double monsters by inverting the egg in the two-celled stage; and, most satisfactory of all, that Herlitzka[62] has succeeded in isolating the first two blastomeres of the newt, each of which then developed into a complete larva.

In Amphioxus Wilson[63] was unable to obtain a gastrula from a $\frac{1}{8}$ blastomere, though a $\frac{1}{4}$ blastomere readily gastrulated. But in Hydroids Zoja[64] found that even a $\frac{1}{16}$ blastomere, a cell which is probably either 'ectodermal' or 'endodermal,' would give rise to a normal embryo.

In the Echinoderms[65], however, although eventually a normal whole larva results, the segmentation of an isolated blastomere is partial, a curved plate of cells being produced which only gradually rolls up to form a hollow blastula. It is only from $\frac{1}{2}$, $\frac{1}{4}$, or $\frac{1}{8}$ blastomeres that perfectly normal gastrulæ can be obtained; but the $\frac{1}{8}$ gastrulæ are of course developed from either animal or vegetative cells. In this connection—the point which for us is of especial interest—Driesch has shown that a normal pluteus can be formed from the eight animal cells alone, the eight vegetative cells alone, or even the four macromeres alone of the sixteen-celled stage.

The Nemertines, as Wilson[66] has recently stated, resemble the Echinoderms; segmentation of the isolated

[60] *Loc. cit.*

[61] *Arch. Ent.-mech.*, vol. 1, p. 269—303, 1895.

[62] *Arch. Ent.-mech.*, vol. 2, p. 352—366, 1896; vol. 4, 624—654, 1897.

[63] *Journ. Morph.*, vol. 8, p. 579—638, 1893.

[64] *Arch. Ent.-mech.*, vol 1, p. 578—594. 1895; vol. 2, p. 1—31, 1896.

[65] Driesch, H., *Zeitschr. wiss. Zool.*, vol. 53, p. 160—184, 1892; vol. 55, p. 1—62, 1893; *Arch. Ent.-mech.*, vol. 4, p. 75—124, 1897; *Mitt. Zool. Stat. Neapel*, vol. 11, p. 221—253, 1895.

½ or ¼ blastomere is partial, but its ultimate development total. Further, complete Pilidia may be reared from either animal or vegetative fragments.[***]

These forms lead us naturally to the Ctenophora and Mollusca, the extreme term at the other end of our series. Here the segmentation of the isolated blastomeres is, as in the Echinoderms, partial ; the resulting embryo is, however, partial too. In the Ctenophora[**] the product of a ¼ blastomere is a little more than a half-larva, and eventually regenerates the missing half; but in the Mollusca[**]—one of the groups we may notice with a 'mosaic' type of segmentation—not only does each blastomere segment as though the remaining blastomeres were there, but no regeneration of the remaining parts ever takes place.

If we had before us these results alone, it would be easy to draw from them a strong confirmation of the evolutionist theory. When, however, we remember Wilson's experiments on *Nereis*—just another of these 'mosaic' forms—and when further we take into consideration the great mass of evidence which the behaviour of the isolated blastomeres of the other groups presents to us, it is impossible, to say the least, to accord to the preformationist hypothesis a universal validity. On the contrary, this hypothesis finds in this evidence, as Driesch puts it, its formal contradiction.

[**] *Arch. Entw. mech.*, vol. 16, p. 411–458, 1903.

[***] According to Zeleny, larvæ from ½ animal blastomeres are devoid of an archenteron, from ¼ vegetative blastomeres of an apical organ (*Journ. Exp. Zool.*, vol. 1, p. 293–320, 1904).

[*] Chun, C., "Festchr. f. Leuckart," Leipzig, 1892, p. 77–108. Driesch, H. and Morgan, T. H., *Arch. Entw.-mech.*, vol. 2, p. 204–224, 1896.

[**] Crampton, H. E., *Arch. Entw.-mech.*, vol. 3, p. 1–10, 1896. The results recently obtained by Wilson on *Dentalium* and *Patella* are similar (*Journ. Exp. Zool.*, vol. 1, p. 1–70, 107–206, 1904).

These clearly are cases in which, at certain stages, the blastomeres are interchangeable, their destinies as yet not fixed; in determining their fate other factors, their mutual position or rather the influence they exert upon one another and—as experiment is abundantly showing—the conditions of their environment, come into play.

And what experiment has thus shown to be true of the individual finds an obvious parallel in nature in the formation, under the stress of varying internal or external causes, of homologous organs from cells or layers of unlike origin. In the one case as in the other the same end is attained by paths that are diverse; one of these may be a recapitulation; all cannot.

On all sides then—and we have now examined the morphological theories of germinal layers from every possible point of view—the facts forbid us to see in these elementary organs of the embryo that definite predetermination for the performance of certain ontogenetic functions which the hypotheses we have been criticising demand. The germinal layers are not sets of cells of universally identical origin which necessarily and invariably give rise to certain fixed parts of the adult organization, but merely convenient terms for the primordia of the structures of the adult.

Similarly constituted ova may and do—as we have seen in the various groups of the Vertebrata, and still more clearly in the spiral cleavages of Turbellarians, Mollusca, and Chaetopoda—segment and gastrulate in a precisely similar manner, and give rise to cells which in origin are alike; and this similarity in the segmentation or gastrulation of the ova of related forms we may, if we insist on retaining the word, perhaps still call 'recapitula-

tion,' though it is not a recapitulation of any adult ancestral type, but merely a repetition of similar ontogenetic functions by cells which have inherited a similar structure.[*] In destiny, however, such cells may be exceedingly diverse: 'Furchungsmosaik,' as Driesch has it, 'braucht kein Mosaik der Potenzen zu sein." It is only within comparatively narrow limits that origin and destiny can coincide.

Nor is the failure of embryology to provide an infallible criterion of homology evident in the history of the germ-layers alone. In organogeny we are often reduced to an exactly similar *impasse;* the oviduct of the Elasmobranchs, to take an example, bears the closest resemblance anatomically to the same organ in the air-breathing Vertebrates; and yet developmentally it is totally dissimilar, arising from the pronephric half of the segmental duct, while in the other forms it has nothing to do with any part of the excretory system. Morphologists, as a matter of fact, adopt the embryological evidence when it suits them and ignore it when the facts are inconvenient In discussing that *vexata quæstio* the homology of the ear-bones, for instance, Gegenbaur,[**] while unhesitatingly accepting the equivalence, as based on development, of the Mammalian stapes with the Sauropsidan columella and of both with the hyomandibular of Fishes, just as unhesitatingly passes over as a caenogenetic modification the origin of the Amphibian columella from the auditory capsule

This, of course, is the usual apology offered; it is the one which Haeckel himself suggested to meet difficulties

[*] Such a repetition as that here indicated is what I understand O. Hertwig to mean by his 'Multiplikation des Organgenetischen Grundgesetzes' ("Handbuch der Entwicklungslehre der Wirbelthiere" Einleitung, p. 56, 57, Jena, 1901.

[**] "Vergleichende Anatomie der Wirbelthiere," vol. 1, p. 440, 8vo, Leipzig, 1898.

which he clearly realised, and it may perhaps be admitted when the exceptions are few and the rule general. But when, as in germ-layer development, exceptions to the rules of theory meet us at every turn, when everything has to be explained away as caenogenetic—how much? or rather how little! of the palingenetic is left!

In the germ-layers, at least, between the conflicting alternatives of origin and destiny, there is no *media via*. To cleave to origin is to plunge into a quagmire of absurdities; to follow destiny is to abandon all hope of finding any ultimate criterion in development, and to return to that older conception of morphological similarity or homology which is based simply on identity of anatomical relations extending over a large series of forms in the same stage of their life history.

And in this direction serious embryological thought is steadily trending. Though some still seem to halt between two opinions not a few—notably Driesch, Hertwig, Braem, Child, Conklin, Treadwell, Morgan—have definitely rejected the ontogenetic criterion of homology and refused any morphological significance, any phylogenetic value to the germinal layers.

But though thus divested of the claims falsely set up on their behalf the germ-layers remain, from another point of view, the morphogenetic, structures of paramount importance. The aim of the experimental embryologist is to give a causal account of the sequence of developmental phenomena, regarding development as one of the functions of the organism to be studied by the ordinary physiological methods; and the problems which confront him in this effort are, in the main, two. The first is to describe in accurate terms the influence exerted upon the embryo by its environment; the second is to determine the mutual relations which subsist between the parts of

the embryo and between the parts and the whole; he has to discover, in a word, what are the external and what the internal factors which govern the process of differentiation. Differentiation sets in with the separation of those elementary embryonic organs which we are accustomed to speak of as the layers of the germ; it is in a precise physiological study of these organs that we must look for the clue to one of the greatest of biological problems, the problem of the epigenetic evolution of the complexity of the adult from the apparent simplicity of the fertilized ovum.

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IV. Battack Printing in Java, with Notes on the Malay Kris and the Bornean Sumpitan and Upas Poison.

By JOHN ALLAN.

Received and Read, January 16th, 1906.

In these days of extended foreign travel there are very few parts of the world which at some time or another are not visited by travellers. It was my good fortune to spend the greater part of the year 1905 in journeying through some of the lands which are far out of the beaten track, and in which there is much with which we are unfamiliar. Of the Malay Peninsula, Java, Borneo, and Sumatra, much has been written at various times, but there are still many things of more than ordinary interest pertaining to these far off countries and their inhabitants about which little and, in many cases nothing, is known.

Many of the native industries are certainly worthy of greater attention than has so far been bestowed upon them, and, certainly to us in Manchester, none of these should be of greater interest than the process of Battacking which has been practised by the natives of India and the Further East for centuries, and which exhibits the simplest form which calico-printing can possibly take.

The commonest articles of wearing apparel amongst the 30 million native inhabitants of Java, indeed throughout the whole of Malaysia, are the Battack-printed sarongs and slendangs and pieces of the same printed material are almost universally used as a covering for the head.

April 7th, 1906.

The sarong, as a portion of the attire of both men and women amongst the Malays, is worn in many ways. It is of such a size and shape, that, at the desire of the possessor, it becomes trousers or petticoat, shirt or overall, or even an article of bed-clothing, and it is the only form of bathing-costume which the natives use.

The Malay is a true conservative in the matter of dress, for, centuries ago, the sarong was worn as it is to-day. In the very heart of central Java, there rises in an open plain a massive pyramid of dark-grey stone, a chaos of cupolas and spires, surmounted by a high central dome. This is the Boro-bodoer, probably the oldest Buddist remains in the world. It is an ancient pile, dating back to the 7th or 8th centuries of this era, and on its sculptured walls are to be seen figures of men and women wearing the sarong as it is worn to-day. Whether the sarongs of that time were battacked or not, it is impossible to say, but it is known that a similar method of printing on cloth was in use in Southern India over 500 years ago, the wonderfully coloured "palampoors" of Madras being produced then, and even now, by a process which is a refinement of the present battacking of Java.

Until comparatively recent times the fabrics used for clothing were entirely of native manufacture, but the ease-loving Malay has found the product of Lancashire looms equally satisfactory for his purpose, and the labour which he is saved by using it counts for much in his eyes. In the most out of the way districts one finds Lancashire cottons in common use, but there is still a good deal of the native-made product to be found in the inland districts.

The first stage in the conversion of white cotton into battacked goods is naturally the removal of the size with

which they are impregnated before leaving this country. This is effected by frequent washings in the rivers, and subsequent exposure of the wet pieces to the sun, the washing and exposure being continued until the whole of the starchy and saline matters are completely removed. The pieces are then finally dried, cut up into sarong length, and are ready to be passed into the hands of the printers. Wash-day is looked upon as more or less of a necessary evil in this country, but amongst the Javanese this condition of things is quite reversed, for water seems to possess a fascination for them, a considerable part of their time being spent in the warm streams with which the country abounds; and in washing articles of clothing they always take the opportunity of standing in as deep water as they can conveniently work in.

Before entering into details, I might explain here that there are really two methods of preparing battacked goods. One of these may be described most simply as a process of colour painting, in which the design is actually drawn on the cloth by hand. This work is almost entirely carried out by women, and, as might be expected, involves the expenditure of an enormous amount of time and labour; and, as a result of this, the process is confined to the production of very expensive articles of attire, the wearing of which is restricted to the wealthy, who are a very small proportion indeed of the native population. Since it is almost an impossibility to imitate such work by a mechanical process, I do not intend to enter into any of the details of carrying it out, except to say that the colours are applied by means of small tubes provided with orifices of varying shape, the tubes having a small cup on their end, which forms a reservoir in which the colour is contained. In many cases the process is combined with the second method of battacking, and from the description

which I shall give of that it will easily be seen how such a combination is possible. Details of this form of battacking are to be found in "Die Indische Batikkunst," by Rouffer and Jagnball, a book of 5 vols., of which 4 have up to the present been issued, and which is worthy of the study of those interested in this subject.

The second process of battacking is more one of dyeing than of printing, the whole fabric being immersed in the dye-bath, but prior to this the parts which are not intended to be dyed are protected by a wax preparation, which has been placed on it in such a way as to form a design. In the coarsest cloths this design is made by freehand drawing with a short thick brush, similar to what is used in this country for rough stencilling, the work being usually done by women who sit on the ground with the cloth stretched out on their knees. The wax which is used for the process is a mixture of paraffin wax and beeswax, or a much-adulterated Japan wax which is shipped specially to the country for the purpose. The wax is kept melted in a pot on a small charcoal fire, round which three or four of the women are seated, and to judge from the continuous chatter which is going on, there must be as many subjects for conversation in Malaysia as there are at an afternoon tea-gathering at home.

The amount of work of this class is much less, however, than that where male labour is employed in stamping the resist patterns on the cloth with a metal die, in exactly the same way as the block-printing of calico was carried out before the introduction of the roller printing machine. In this case, the workman, like the women, is seated on the ground, with his legs underneath a large padded board, on which the white cotton is spread out. Having coated the die with wax, he

presses it on the cloth, working continuously from left to right until the whole cloth has been covered and the design is complete.

The blocks which are used are made of thin strips of sheet brass inserted in a wooden back, in such a way that their edges produce the running patterns which are invariably used in battack work. They act in use in exactly the same way as the type does in ordinary letter-press printing. Several blocks are used to work out the complete design, and the work of any particular battacking establishment can be recognised in different patterns of sarong by the appearance here and there of the same block.

The wax used for pattern stamping is a different composition from that employed in the brushwork, as the latter is so thin that it would run too easily from the block and smudge the design. The block wax is a mixture of ordinary resin and paraffin wax, or some of the varnish-gums which are collected by the natives. The wax is melted over a charcoal fire, and after being dipped the block is freed from the excess of wax by being pressed on a pad which is kept warm by being placed in close proximity to the melting fire

Having been completely covered with the design in this way the cloth is hung up for a short time, so that the wax may become thoroughly hard before the process of dyeing is commenced.

So far as I have been able to observe, it is the practice of the Javanese never to use more than two colours in this class of sarong printing, these being indigo and the brown dye obtained from mangrove bark, but by dyeing in both of these colours they are able to produce a black. The result of this limited use of colours is that this battack work is characterised by a white, blue, brown and black

combination. The cloth is first dyed with the indigo, and then before being immersed in the brown dye is freed from the old wax and reprinted with wax on those parts which are to be protected from brown, some of which are the original white, and others the blue, where this is to be left of the pure indigo shade.

The indigo which is used is of native manufacture, and is always in the form of a moist paste. The dye-bath is prepared by diffusing a quantity of this paste in water and reducing it with waste molasses from the sugar factories by fermentation, a process which is similar to the bran fermentation vat so largely used in wool dyeing in our own country. In another factory, I have seen, the solution is kept alkaline with a mixture of wood ashes and quicklime and gently warmed. The preparation of the indigo bath in this way is interesting, as it is similar to a method used in this country for fixing indigo in printing, by means of a reducing mixture of glucose and caustic soda.

To obtain the full depth of blue that is required it is usual to dip and expose the cotton to the air several times, the process therefore taking some little time to complete.

In preparing the brown dye the mangrove bark is first broken into small pieces, and then boiled for some time with water, so as to obtain the whole of the soluble extractive matter. After removing the bark, the solution is concentrated to a very thick brownish black syrup, and kept in stock for use as required. As a rule no mordant is used with this brown dye, which is essentially of the cutch type, although in this country when dyes of this character are used, both copper sulphate and bichromate of potash are employed as fixing agents. To fix this dye the natives simply expose the fabric to the air for some

time, the process of dipping and exposing being continued until the required shade has been obtained. The dyed material is then freed from wax by thorough washing in wood ashes, and after being ironed becomes a finished sarong or slendang.

Many sarongs are now printed in this country and also in Holland, but the native still prefers the home-made article, and is prepared to pay double the price for it, that he will pay for an imported one. Perhaps his faith in the superiority of the locally produced article is not unjustified, for he has had frequent experience of the fact that the colours of European-made sarongs are of a very fleeting character, and it must be admitted that a colour which will stand constant wetting and exposure to a tropical sun must be fast in more than the ordinary sense. If the trade in sarongs exported from this country is to increase, this point must be more fully attended to than it has been in the past.

Malay Kris.

To mention the Malay Kris is to recall at once countless acts of piracy and bloodshed in Eastern seas, in which this weapon has been used with deadly effect. The name is invariably associated with a zig-zag shaped double-edged dagger, from 12 to 16 inches long, but, though this form is very frequently met with, it is not the only one in which the kris is made. The zig-zag kris is purely a weapon, but in the countries throughout Malaysia where the kris is so universally carried, the native finds it necessary, or, at any rate, a source of great convenience to use his weapon as a cutting tool, and for this reason, the kris is frequently made with a straight blade, with or without a double cutting edge. The single-edged blades, however, are usually heavier and longer than the true

kris, and are distinguished from it by the name of kulewang. For jungle-clearing, there is probably no better tool than the kulewang if the Bornean parang ilang be excepted.

The kris blade is usually characterized by beautifully damascened markings, though many plain blades are met with, but these are not esteemed so highly by the natives, since the plain blades are not so strong, nor is the labour entailed in their manufacture so great as is the case with the damascened blade.

The kris is fitted with a wood or bone handle, usually carved into grotesque shapes of men or animals, but with no hand-guard other than a triangular widening-out of the base of the blade.

In using the weapon, a favourite stroke is an upward blow with the point entering beneath the shoulder blade or a downward stroke with the weapon thrust between the collar bone and first rib.

Until comparatively recent times, the metal which was used for kris manufacture was entirely native iron, but of recent years, with the extension of commercial relationships with European countries, nails, hoop-iron and other forms of scrap have come to be employed—not, however, it must be acknowledged, to the improvement of the weapon. How long iron has been made in Malaysia it is impossible to say, but the process of manufacture is similar to that still used in India and China, and it is probable that the art was imported from one or other of these two countries, as there has for a long time been a close commercial association between them and the Malay Archipelago.

The furnaces in which iron is at present made by the natives are built up from clay to a height of 3 or 4ft., and from 8 to 10ft. in diameter, the whole furnace

being bound round with rattan or split bamboo to prevent its coming to pieces, owing to the extensive cracking which takes place when it is in use. At the top of the furnace, the walls are about 2ft. thick, but the square or circular opening of 4 or 5ft. diameter gradually tapers to about 1½ to 2ft. at the hearth. A blast of air is supplied to the furnace from two cylinders of wood fitted with feather-edged pistons, to which rods are attached, and which a workman operates by sitting between the cylinders and working the pistons alternately up and down. The blast is carried in bamboo tubes and is admitted to the furnace through clay tuyers, which are formed by moulding clay round small pieces of bamboo and then burning the bamboo out in a fire, whereby the clay is at the same time baked.

The ore which is used consists of nodules of hæmatite usually obtained from a river-bed, or else a clay iron stone, which is surface mined. Both of these ores are roasted in wood fires and are then charged into the furnace with a large excess of fuel. No flux of any kind is used in the charge, and as is always the case where this is not done, the process is a very wasteful one since the impurities in the ore are fluxed off by combination with oxide of iron which otherwise would have been reduced to metal. The slag is drawn off at regular intervals, and in the end a spongy mass of about 100lbs. of steely iron is obtained, the quality of which is determined by the proportion of fuel to ore in the original charge, and the temperature which the furnace is allowed to acquire in working.

The mass of iron is freed from slag by hammering, but always retains some which shows itself in the form of small flaws in the metal, when this is worked up.

A plain kris is made by simple forging from the iron thus obtained, but the damascened form is made by

welding together selected pieces of metal which approximate to our wrought iron and soft steel in composition. No less than 5 strips of metal are welded together to make the blade, the central and two outer ones being of steel and the two others of wrought iron, the strips of this metal having previously been bent up into a kind of serpentine ribbon. The various pieces of metal are welded together at one operation, and the welding temperature is such that the more fluid steel is forced between the corrugations of the wrought iron ribbons producing the damascened structure which is so much desired. From this compound piece of metal the blade is shaped by rubbing down on a stone, and finally, to more fully develop the damascening, it is pickled by being boiled for two or three days in rice water to which some sulphur and nitre obtained from the soil have been added.

The sheath for the kris is formed from solid wood and has a large curved cross-piece at the top resembling a sword guard, which, owing to its size and shape, enables the wearer to carry the weapon in the folds of his sarong without cords or straps of any kind.

BORNEAN SUMPITAN AND UPAS POISON.

It is impossible to take up any book describing the habits and customs of the Dyaks of Borneo without finding some mention of the deadly poisoned darts of the sumpitan or blowtube which is universally employed by them both as a means of obtaining food and in their many intertribal wars.

The sumpitan is a tube formed from a hard brownish black wood called by the Dyaks "tapang" and is about 7 or 8 feet long having an external diameter of about an inch and an internal diameter of about a quarter of an

inch. A peculiar method is adopted for boring the tube of the sumpitan. The wood having been carefully selected is roughly hewn into a cylinder of the required length but usually at least three inches in diameter and is then hung up in a vertical position in the centre of a stout four cornered scaffolding in such a way that its lowest end is just over the workman's head. The tool which is used for boring consists of a long iron rod with a chisel-edged end, which the workman directs with upward strokes against the centre of the hanging log. The iron drill is fitted with a moveable wooden handle which is moved downwards as the hole is driven further and further into the log. The operation, as might be expected, is a slow one, but in spite of the primitive tool which is employed and the method of using it, it is astonishing how accurately the centering of the hole is maintained. To finally smooth and polish the tube a piece of rattan, which closely fits it, is passed through it and this is worked backwards and forwards until the required finish is obtained. The rough blowtube is now carefully cut down to the required thickness and the exterior rounded and smoothed off by scraping. Usually the sumpitan is used as a spear as well as a blow tube, a heavy spear head about 12 or 14 inches long being lashed to the end with thin strips of rattan in such a way that there is no interference with its use as a means of projecting the small darts of which the native carries a supply in a bamboo case slung at his waist.

The darts are seemingly harmless weapons, since they are merely small pieces of the central stem of a palm leaf from six to eight inches long and no thicker than an ordinary knitting needle. One end of the dart is fitted with an inch-long plug of soft pith which closely fits the tube of the sumpitan, whilst the pointed end is

smeared over for about an inch of its surface with the poison.

There are two kinds of the so-called Upas or more correctly Ipo poison in use in Borneo, but the one most frequently employed is prepared from the sap of the *Antiaris toxicaria* or Upas tree, whose leaves are popularly supposed to distil a poisonous dew and under whose shade it is fatal to rest. Needless to say these beliefs have not the slightest foundation in truth, but of the deadly effects of the prepared poison there is not the slightest doubt. Injected into the system by means of an arrow, sickness rapidly comes on followed by convulsions and death. It has fortunately not been my lot to see its effect upon the human subject, but birds fall almost as if they had been shot with a gun and a wild pig pierced with only one arrow was dead in less than 20 minutes after being struck.

To prepare the poison the native makes an incision in the bark of the tree and collects the white milky sap on a plantain leaf. This is exposed to the sun until it has been concentrated to a thick brown syrup, after which the leaf is folded up and hung above one of the cooking fires until further evaporation has reduced the sap to a sticky brown mass. In this state the poison can be kept for some time and when wanted for use is dissolved in the juice of the "tuba" root, well known for its property of stupefying fish, or failing this, then tobacco juice or even lemon juice may be employed, the dried poison being mixed with the solvent to the consistency of a thin paste into which the arrows or weapons to be poisoned are dipped.

The active principle of the poison is the glucoside antiarin, the name of this as well as of the tree being derived from the native name of the poison, Upas Antjar,

which distinguishes it from the other arrow poison, Upas Kajah or sometimes Tjettik. Of the manufacture of the latter poison and its properties, I can only report information given to me by the natives as I have neither seen it made or employed. I have been told that it is prepared by boiling Tjettik roots with water and from the description of its effects it must be somewhat similar to the South American curare poison which is of the strychnine family.

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V. Report on the Recent Foraminifera from the Coast of the Island of Delos (Grecian Archipelago). Part III.

By HENRY SIDEBOTTOM.

Received and read February 13th, 1906.

LAGENIDÆ.

LAGENINÆ.

Lagena, Walker and Boys.

***Lagena globosa,** Montagu, sp.

Entosolenia globosa (Montagu), Williamson ('58), p. 8, pl. 1, figs. 15, 16.

Lagena globosa (Montagu), Reuss ('63), p. 318, pl. 1, figs. 1—3.

Lagenulina globosa (Montagu), Terquem ('76), p. 67, pl. 7, figs. 3, 4.

Lagena globosa (Montagu), Brady, ('84), p. 452, pl. 56, figs. 1—3.

Many of the specimens are nearly globular in shape, others more or less pyriform and irregular. Frequent.

*There are present a number of tests that are elongate, more or less pyriform, slightly compressed, with stellate aperture and no entosolenian tube. Mr. Millett, in his Malay report, suggests that many of these forms are nothing more nor less than arrested growths of *Nodosaria* and *Polymorphina.*

* The asterisk denotes that this species occurs at Palermo.

April 19th, 1906.

***Lagena botelliformis**, Brady, var. (Pl. 1, fig. 1).

Lagena botelliformis, Brady ('84), p. 454, pl. 56, fig. 6.

The contour of the Delos specimens so closely resembles Brady's figure in the above reference, with the exception that the orifice is situated at the end of a produced neck, that I think it may be considered a variation of that species, in preference to treating it as a variety of *L. laevis*. *L. laevis* does not occur in the material examined. Six specimens were found, only one at Palermo. Very rare.

***Lagena ampulla-distoma**, Rymer Jones (Pl. 1, figs. 2, 3).

Lagena vulgaris, var. *ampulla-distoma*, Ry. Jones ('72), p. 63, pl. 19, fig. 52.

L. ampulla-distoma (Ry. Jones), Brady ('84), p. 458, pl. 57, fig. 5.

L. ampulla-distoma (Ry. Jones), Millett (:01), p. 5, pl. 1, fig. 5.

This occurs in three forms. In the four largest specimens the test is roughened all over, as in fig. 2; in the smaller ones only half of the test is rough, and the shell deposit takes the form of very short blunted spines, which have a tendency to coalesce and run into lines as they approach the clear part of the shell, as in fig. 3. Whilst in the smallest examples (and they are very minute) the shell is covered with protuberances which show a still greater tendency to run into lines—in fact, in one case they have nearly done so. All the forms are very rare. More frequent at Palermo.

***Lagena striata**, d'Orbigny, sp.

Oolina striata, d'Orbigny ('39), p. 21, pl. 5, fig. 12.

L. striata (d'Orb.), Brady ('84), p. 460, pl. 57, figs. 19, 22, 24, 28—30.

Lagena striata (d'Orb.), Brady, Parker and Jones ('88), p. 222, pl. 44, fig. 28.

The examples are small, and the striæ become very faint between the body of the test and the neck. More typical examples are found at Palermo, where this species is also more plentiful.

***Lagena sulcata,** Walker and Jacob, sp.

Lagena striata, Williamson ('48), p. 13, pl. 1, figs. 6, 8.

L. vulgaris, var. *perlucida*, Williamson ('58), p. 5, pl. 1, fig. 8.

L. vulgaris, var. *striata*, Williamson ('58), p. 6, pl. 1, fig. 10.

L. sulcata (W. & J.) Brady ('84), p. 462, pl. 57, figs. 23, 26. Very frequent.

***Lagena sulcata,** var. **interrupta,** Williamson.

Lagena striata, var. *interrupta*, Williamson ('48), p. 14, pl. 1, fig. 7.

L. vulgaris, var. *interrupta*, Williamson ('58), p. 7, pl. 1, fig. 11

L. sulcata, var. *interrupta* (Williamson), Brady ('84), p. 463, pl. 57, figs. 25, 27. Frequent.

***Lagena semistriata,** Williamson. (Pl. 1, figs. 4, 5.)

Lagena striata, var. β, *semistriata*, Williamson ('48), p. 14, pl. 1, figs. 9, 10.

L. vulgaris, var. *semistriata*, Williamson ('58), p. 6, pl. 1, fig. 9. Very frequent.

The Delos examples of these three forms run into each other. Fig. 5 shows how closely some specimens of *L. semistriata* approach *L. laevis*, the striæ being represented solely by very minute tubercles on the bottom of the test. *L. laevis* not being represented in the Delos

welding together selected pieces of metal which approximate to our wrought iron and soft steel in composition. No less than 5 strips of metal are welded together to make the blade, the central and two outer ones being of steel and the two others of wrought iron, the strips of this metal having previously been bent up into a kind of serpentine ribbon. The various pieces of metal are welded together at one operation, and the welding temperature is such that the more fluid steel is forced between the corrugations of the wrought iron ribbons producing the damascened structure which is so much desired. From this compound piece of metal the blade is shaped by rubbing down on a stone, and finally, to more fully develop the damascening, it is pickled by being boiled for two or three days in rice water to which some sulphur and nitre obtained from the soil have been added.

The sheath for the kris is formed from solid wood and has a large curved cross-piece at the top resembling a sword guard, which, owing to its size and shape, enables the wearer to carry the weapon in the folds of his sarong without cords or straps of any kind.

BORNEAN SUMPITAN AND UPAS POISON.

It is impossible to take up any book describing the habits and customs of the Dyaks of Borneo without finding some mention of the deadly poisoned darts of the sumpitan or blowtube which is universally employed by them both as a means of obtaining food and in their many intertribal wars.

The sumpitan is a tube formed from a hard brownish black wood called by the Dyaks "tapang" and is about 7 or 8 feet long having an external diameter of about an inch and an internal diameter of about a quarter of an

inch. A peculiar method is adopted for boring the tube of the sumpitan. The wood having been carefully selected is roughly hewn into a cylinder of the required length but usually at least three inches in diameter and is then hung up in a vertical position in the centre of a stout four cornered scaffolding in such a way that its lowest end is just over the workman's head. The tool which is used for boring consists of a long iron rod with a chisel-edged end, which the workman directs with upward strokes against the centre of the hanging log. The iron drill is fitted with a moveable wooden handle which is moved downwards as the hole is driven further and further into the log. The operation, as might be expected, is a slow one, but in spite of the primitive tool which is employed and the method of using it, it is astonishing how accurately the centering of the hole is maintained. To finally smooth and polish the tube a piece of rattan, which closely fits it, is passed through it and this is worked backwards and forwards until the required finish is obtained. The rough blowtube is now carefully cut down to the required thickness and the exterior rounded and smoothed off by scraping. Usually the sumpitan is used as a spear as well as a blow tube, a heavy spear head about 12 or 14 inches long being lashed to the end with thin strips of rattan in such a way that there is no interference with its use as a means of projecting the small darts of which the native carries a supply in a bamboo case slung at his waist.

The darts are seemingly harmless weapons, since they are merely small pieces of the central stem of a palm leaf from six to eight inches long and no thicker than an ordinary knitting needle. One end of the dart is fitted with an inch-long plug of soft pith which closely fits the tube of the sumpitan, whilst the pointed end is

smeared over for about an inch of its surface with the poison.

There are two kinds of the so-called Upas or more correctly Ipo poison in use in Borneo, but the one most frequently employed is prepared from the sap of the *Antiaris toxicaria* or Upas tree, whose leaves are popularly supposed to distil a poisonous dew and under whose shade it is fatal to rest. Needless to say these beliefs have not the slightest foundation in truth, but of the deadly effects of the prepared poison there is not the slightest doubt. Injected into the system by means of an arrow, sickness rapidly comes on followed by convulsions and death. It has fortunately not been my lot to see its effect upon the human subject, but birds fall almost as if they had been shot with a gun and a wild pig pierced with only one arrow was dead in less than 20 minutes after being struck.

To prepare the poison the native makes an incision in the bark of the tree and collects the white milky sap on a plantain leaf. This is exposed to the sun until it has been concentrated to a thick brown syrup, after which the leaf is folded up and hung above one of the cooking fires until further evaporation has reduced the sap to a sticky brown mass. In this state the poison can be kept for some time and when wanted for use is dissolved in the juice of the "tuba" root, well known for its property of stupefying fish, or failing this, then tobacco juice or even lemon juice may be employed, the dried poison being mixed with the solvent to the consistency of a thin paste into which the arrows or weapons to be poisoned are dipped.

The active principle of the poison is the glucoside antiarin, the name of this as well as of the tree being derived from the native name of the poison, Upas Antjar,

pl. 7, fig. 11), two curved bands appear on each side of the shell. In *L. faba*, Balkwill and Millett ('84, p. 81, pl. 2, fig. 10), these bands are slightly raised, whilst they become costæ in *L. quadricostulata*, Reuss, *L. fasciata*, Egger, and *L. meyeriana*, Chapman ('94, p. 706, pl. 34, fig. 7). These bands may or may not unite at the base of the shell; Dr. Egger's examples show both conditions, whilst in the only known specimen of *L. meyeriana*, the costæ, although continuous, are recurved, and form a sinus at the aboral extremity."

The peculiarity of the Delos examples is that the grooves, or shallow sulci, have their edges raised above the body of the shell.

In the varieties figs. 14, 15, 16, 17, this feature is well marked, whilst in fig. 13 there is a doubt as to its being present, owing to the fineness of the markings, and I have been unable to satisfy myself on this point. In only two or three cases are the grooves continuous at the base of the shell. This range, with few exceptions, has a short entosolenian tube which is straight, and therefore not attached to either face of the test. This tube is often split at the end, instead of having the well-known trumpet orifice.

The Delos forms are evidently closely allied to *L. annectens*, Burrows and Holland, and future research may connect them with *L. alveolata*, Brady. Rare to very rare. Only the forms represented by figs. 14, 15, 16, occur at Palermo.

Lagena fasciata, Egger, var. **carinata**, nov. (Pl. 1, fig. 17).

This variation has a central keel; the test is very finely pitted all over, and so has not the same transparency as we find in the other varieties. A short, stout, straight

hood, and is rare. *Fig. 22 has faintly frosted-looking bands as in *L. lucida* and is very rare. Pl. 2, fig. 1, has the fine lines running round the edges of the test as in some varieties of *L. fasciata*, also very rare. *Fig. 2 has a short neck, sides not quite parallel, and perhaps might be treated as a variety of *L. laevigata*. *Fig. 3 is partially carinate, and agrees very closely with Brady's "Challenger" drawing ('84, pl. 59, fig. 16). This form in the Delos gatherings is the most numerous of the quadrate varieties, but I hardly think it worthy of a varietal name. Frequent.

***Lagena marginata,** Walker and Boys.

Oolina compressa, d'Orbigny ('39), p. 18, pl. 5, figs. 1, 2.

Fissurina carinata, Reuss ('62), p. 338, pl. 6, fig. 83, pl. 7, fig. 86.

Lagena marginata (W. & B.), Silvestri ('96), p. 119, pl. 3, figs. 7—9.

L. marginata (W. & B), Flint ('99), p. 307, pl. 54, fig. 2.

Most of the examples are small, nearly circular in outline, and have the keel fairly well developed. Frequent. *There is a variety present, which comes very near to fig. 27 on pl. 44, Brady, Parker, and Jones ('88) It is placed by them under *L. marginata*, but it appears to me to be intermediate between *L. marginata* (W. & B.) and *L. marginata*, var. *semimarginata*, Reuss.

Lagena squamoso-marginata, Parker and Jones (Pl. 2, fig. 4).

Lagena squamoso-marginata, Parker and Jones ('65), p. 356, pl. 18, fig. 2.

L. squamoso-marginata (P. & J.), Brady ('84), p. 481, pl. 60, fig. 24.

L. squamoso-marginata (P. & J.), Millett (:01), p. 622, pl. 14, fig. 7.

All the specimens have a single keel. The entosolenian tube is attached. The mouth opens out at the lower edge of the keel, and the hexagonal markings are raised. About thirty found. Rather rare.

***Lagena marginato-perforata,** Seguenza (Pl. 2, fig. 5).

Lagena marginato-perforata (Seg.), Millett (:01), p. 621, Pl. 14, fig. 4.

This elegant foraminifer is frequent in these gatherings. The keel varies in its development, in some cases it scarcely shows, in others it springs from the edge of the mouth and is continuous right round the test, being widest at the aboral end. The internal tube is straight and free. In all the examples the centre of the test is unornamented. One specimen of trigonal form occurs.

***Lagena marginata** (W. & B.), var. **inaequilateralis** Wright (Pl. 2, fig. 6).

Lagena marginata (W. & B.), var. *inaequilateralis*, Wright, ('86) p. 321, pl. 26, fig. 10.

The specimens are quite typical, beautifully clear, and in all cases the internal tube is attached to the test; the mouth is peculiar and agrees with the Irish examples. Frequent.

Lagena inaequilateralis (Wright), var. **semi-marginata,** nov. (Pl. 2, fig. 7).

This is an interesting variation of *L. marginata* var. *inaequilateralis*, Wright. The mouth is the same as in

Mr. Wright's form, and the test is also inequilateral. The keel however is confined to the aboral end of the test and is well developed. Six specimens found. Very rare.

Lagena irregularis, n. sp. (Pl. 2, fig. 8).

One face of the test is bent back in the centre at about an angle of forty five, the opposite side being highly convex ; periphery bi-carinate. Aperture, a small slit situated between the two edges of the keels. Entosolenian.

This curious form is difficult both to describe and to draw quite satisfactorily. It is very minute, and reference to the figures will best explain its peculiarities. The keels are well developed at the oral end, whilst at the aboral they are not so wide, but the bicarinate condition is more pronounced. The space between the keels, small as it is, is partially filled with débris, or shell-growth, and this increases the difficulty of examination. The internal tube in all cases runs along the centre of the back until it nearly reaches the other end, where it turns to one side The orifice is a small slit, apparently situated between the edges of the keels as shown in the drawings, but it is very difficult to make out.

It has been thought that this form might be a distorted or wild-growing example of *L. marginata*, var. *inaequilateralis*, Wright, but the twelve specimens found are all alike. The mouth of the test is not protruded as in Mr. Wright's figures of his species, and as in the Delos specimens of the same form, in addition to which the keel is double. Rare.

***Lagena lagenoides,** Williamson, sp.

Entosolenia marginata, var. *lagenoides*, Williamson ('58), p. 11, pl. 1, figs. 25, 26.

Lagena lagenoides (Williamson), Reuss ('62), p. 324, pl. 2, figs. 27, 28.

L. lagenoides (Williamson), Balkwill and Millett ('84), p. 82, pl. 2, fig. 11.

The specimens agree best with Williamson's fig. 26. Frequent. *A smaller form is also present, in which the mouth only slightly protrudes, extending nearly the whole width of the test. Mr. Wright, of Belfast, Ireland, considers this to be *L. ornata*, Williamson, which is a form of *L. lagenoides*. Rare.

* **Lagena lagenoides**, var. **tenuistriata**, Brady.

(Pl. 2, figs. 9, 10.)

Lagena lagenoides, var. *tenuistriata*, Brady ('84), p. 479, pl. 60, figs. 11, 15, 16.

L. lagenoides, var. *tenuistriata* (Brady), Balkwill and Millett ('84), p. 82, pl. 2, fig. 12.

In the Delos specimens, the centres of the tests are sometimes free from striæ. The entosolenian tube is short and straight. Frequent.

* **Lagena orbignyana**, Seguenza, sp. var. (Pl. 2, fig. 11.)

Entosolenia marginata (pars.), Williamson ('58), p. 9, pl. 1, figs. 19, 20.

Lagena orbignyana (Seguenza), Brady ('84), p. 484, pl. 59, figs. 1, 18, 24-26.

L. orbignyana (Seguenza), Brady, Parker and Jones ('88), p. 222, pl. 44, fig. 20.

L. orbignyana (Seguenza), Flint ('99), p. 308, pl. 54, fig. 4.

The test in this variety is slightly twisted; and in the middle, on either face, is an oval ridge of clear shell-substance. The internal tube is very much curled. In one or two cases the central keel is split and filled up

with débris. Most of the specimens are badly fractured at the edge of the test. Rather rare.

Lagena orbignyana ? var. **falcata**, nov. (Pl. 2, fig. 12.)

The test is compressed, and has two recurved spines, springing respectively from either side of the shell, near the orifice. The fine line running on each side of the delicate keel (or very angular margin) is continuous except at the oral end of the shell. The mouth is oval and slightly produced. The internal tube is short and free. Mr. Millett kindly drew my attention to Dr. Chaster's *Lingulana herdmani* '92, pl. 1, fig. 9), a detached chamber of which would have a strong resemblance to the Delos specimens, but these latter bear no evidence of fracture. They could hardly be the initial chambers of Dr. Chaster's species, as his examples bear no spines on the initial chamber. The lines above referred to are also absent in *Lingulina herdmani*. The two specimens found are exactly alike. Very rare.

Lagena bicarinata, Terquem, sp. (Pl. 2, figs. 13, 14, 15.)

Fissurina bicarinata, Terquem ('82), p. 31, pl. 9, fig. 24.

Lagena bicarinata (Terquem), Balkwill and Millett ('84), p. 82, pl. 2, fig. 4.

L. bicarinata Terquem, Balkwill and Wright ('85), p. 342, pl. 12, fig. 30.

L. bicarinata Terquem), Wright ('86), p. 326 pl. 26, fig. 8.

L. bicarinata (Terquem), Halkyard '89, p. 65, pl. 2, fig. 1.

L. bicarinata Terquem Millett ('01), p. 624, pl. 14, fig. 13.

Three varieties occur which have the two keels, and although very different in other respects, I have brought them together under the above heading. Fig. 13 has the two keels well developed, but has no neck, and is of frequent occurrence. Fig. 14 has a well developed neck with phialine lip surrounding the orifice, also a ridge on either side encircling the body of the test, and is rare. Fig. 15 has the keels very poorly developed and is frequent. All the varieties have the internal tube attached to one face of the test.

Lagena orbignyana, var. **clathrata,** Brady (Pl. 2, fig. 16).

Lagena clathrata, Brady ('84), p. 485, pl. 60, fig. 4.

L. clathrata (Brady), Balkwill and Millett ('84), p. 82, pl. 2, fig. 14.

L. orbignyana, var. *clathrata* (Brady), Millett (:01), p. 628, pl. 14, fig. 23.

The examples agree with the form figured by Messrs. Balkwill and Millett in the above reference. Frequent.

***Lagena fimbriata,** Brady.

Lagena fimbriata, Brady ('84), p. 486, pl. 60, figs. 26—28.

L. fimbriata (Brady), Balkwill and Millett ('84), p. 82, pl. 2, fig. 5.

Brady, in the "Challenger" Report, speaks of this as being a deep-water form, but it has been found both by Mr. Millett and Mr. Wright in shallow-water off the coast of Ireland. The two or three specimens found at Delos agree well with the Irish forms, although in the former the oval wing surrounding the base is not well developed and is more compressed. Very rare. Better examples occur at Palermo.

***Lagena alveolata,** Brady (Pl. 2, fig. 17).

Lagena alveolata, Brady ('84), p. 487, pl. 60, figs. 30, 32.

In the above reference, Brady states that *L. alveolata* is only found in deep water, and reports its occurrence in the North Atlantic, 2,750 fms.; in the South Atlantic, 2,200 fms.; in the Southern Ocean, 2,600 fms.; in the South Pacific; and in the North Pacific, 2,300 fms.

The depth at which the Delos examples were found varied from 8 to 14 fms. The tests are very transparent, and all have a long entosolenian tube attached to one face of the shell. About 15 were found. Rare.

***Lagena protea,** Chaster (Pl. 2, fig. 18).

Lagena protea, Chaster ('92), p. 62, pl. 1, fig. 14.

There are about fifteen specimens of this protean form, no two of which are alike. I have one specimen attached to a piece of shell, and probably the one figured has likewise been adherent. Rare. Still finer examples were found in the Palermo material.

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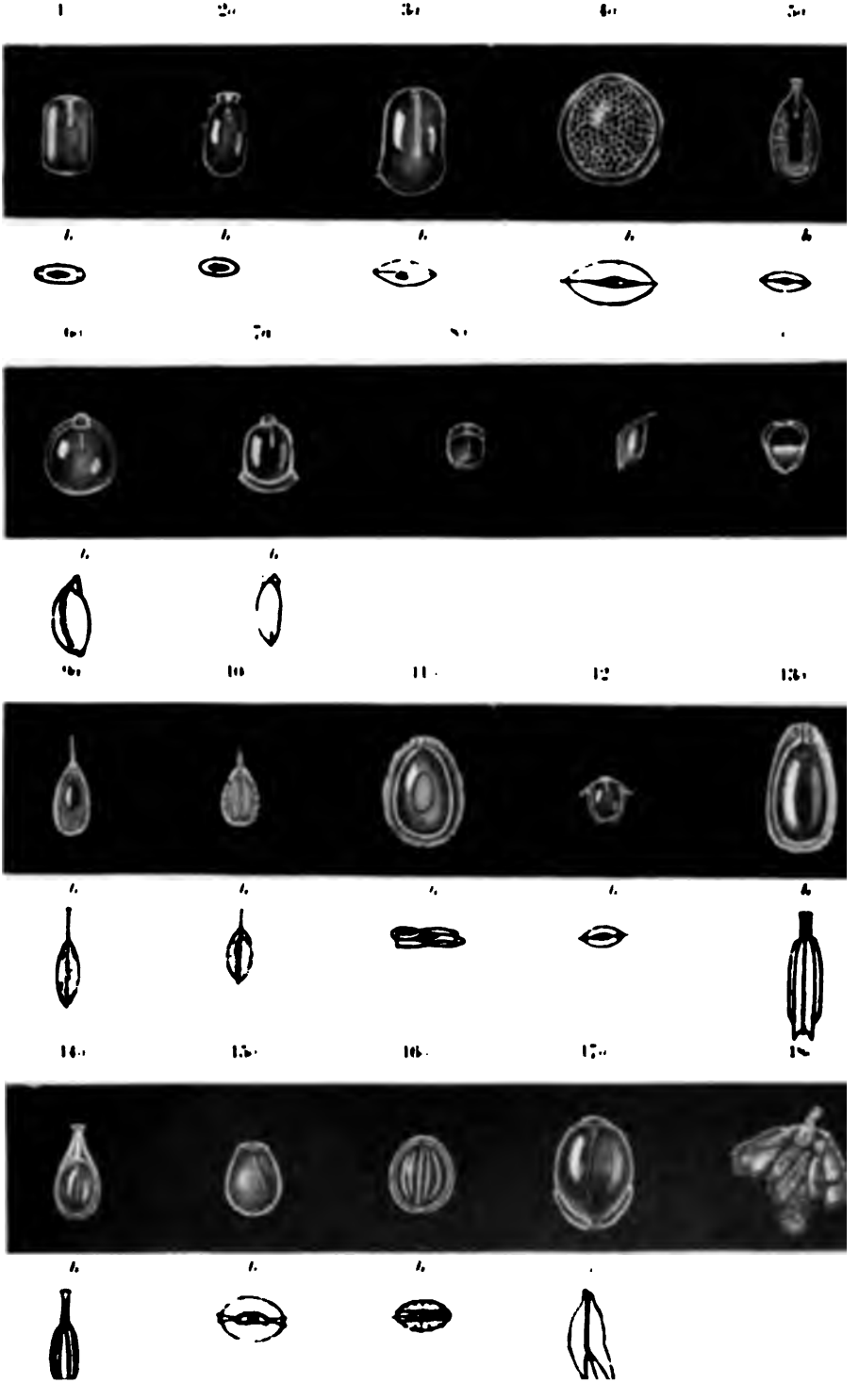
EXPLANATION OF PLATES.

PLATE I.

FIGS.				PAGE
1.	*Lagena botelliformis*, Brady, var.	× 50 ...		2
2, 3.	,, *ampulla-distoma*, Ry. Jones, Fig. 2 × 50, Fig. 3	× 75 ...		2
4, 5.	,, *semistriata*, Williamson	× 75 ...		3
6.	,, *laevigata*, Reuss	× 75 ...		5
7, 8.	,, *laevigata*, Reuss, var. *acuta*, Reuss	× 75 ...		6
9-12.	,, *lucida*, Williamson	× 75 ...		6
13-16.	,, *fasciata*, Egger	× 75 ...		6
17.	,, *fasciata*, Egger, var. *carinata*, nov.	× 75 ...		7
18-20.	,, *staphyllearia*, Schwager	× 75 ...		8
21, 22.	,, *quadrata*, Williamson	× 75 ...		8

PLATE II.

1-3.	*Lagena quadrata*, Williamson	× 75 ...		8
4.	,, *squamoso-marginata*, Parker and Jones	× 100 ...		9
5.	,, *marginato-perforata*, Seguenza	× 75 ...		10
6.	,, *marginata* (W. & B.), var. *inaequilateralis*, Wright	× 75 ...		10
7.	,, *inaequilateralis* (Wright), var. *semi-carinata*, nov.	× 75 ...		10
8.	,, *irregularis*, n. sp.	× 75 ...		11
9, 10.	,, *lagenoides* (Will.), var. *tenuistriata*, Brady	× 75 ...		12
11.	,, *orbignyana*, Seguenza	× 75 ...		12
12.	,, *orbignyana*? (Seg.) var. *falcata*, nov.	× 75 ...		13
13-15.	,, *bicarinata*, Terquem	× 75 ...		13
16.	,, *orbignyana* (Seg.), var. *clathrata*, Brady	× 75 ...		14
17.	,, *alveolata*, Brady	× 75 ...		15
18.	,, *protea*, Chaster	× 25 ...		15



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VI. The Cytological Aspect of Parthenogenesis in Insects.

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The cytological aspects of the biological phenomenon Parthenogenesis, that is the changes which take place in the maturation and development of the unfertilized egg, have only been studied within comparatively recent years, though our knowledge of the occurrence of the phenomenon dates back to the time of Aristotle.

At the present stage of the inquiry, it appeared to me that a useful purpose would be served if a short summary were made of our present knowledge of the cytological phenomena associated with parthenogenesis in that group of animals—the Insects, in which the problem was first studied, and in which examples of the different types of parthenogenesis are found.

For the sake of brevity and clearness, little reference will be made to the general parthenogenetic phenomena, as these are fairly well known and have been summarised recently by Phillips (78). Since his review was written, our knowledge of the maturation of the parthenogenetic ovum has been increased, chiefly by researches made with a view to investigating the chromosomes, the study of which is occupying an ever-increasing number of workers. We shall have more light thrown upon those great biological problems — Sex, Fertilisation, and Heredity, about which, the last especially, we know

May 21st, 1906.

very little at present, by the careful study of parthenogenesis in all its aspects.

Parthenogenesis occurs in the majority of the orders of Insects, and in this group we can recognise all the types of the phenomenon, as they are known from a study of the general or somatic changes which take place.

To obviate the necessity of explaining at length in each case the type of parthenogenesis which occurs in that particular family which we may be considering, it will be convenient if they are arranged in the following tabular form, which is a slight modification of Henneguy's (45) classification :—

1. *Tychoparthenogenesis* (Henneguy).

   This includes those cases of Parthenogenesis which are accidental and of exceptional occurrence, such as are found in the Lepidoptera and Coleoptera.

2. *Homoparthenogenesis* (Henneguy).

   Under this term which replaces Hatschek's term Isoparthenogenesis, all cases of normal Parthenogenesis are included. They may be further divided into

   (*a*) *Thelyotoky.*

   Parthenogenesis in which only females are produced. This occurs in some species of Tenthredinidæ.

   (*b*) *Arrhenotoky.*

   Parthenogenesis in which only males are produced. This occurs in the Hymenoptera Sociales and some species of Tenthredinidæ. (The workers of some Ants, Bees, and Wasps may produce parthenogenetic eggs, though this is only occasional, but in all cases they produce males).

(*c*) *Deuterotoky.*

Both sexes are produced as in some species of Tenthredinidæ. (Deuterotoky also occurs in the next division—Heteroparthenogenesis).

3. *Heteroparthenogenesis* (Henneguy).

Parthenogenesis in which there is an alternation of generations, a cyclical alternation of sexual and parthenogenetic forms. This may be either

(*a*) *Regular* as in the Cynipidæ and Aphidæ, or

(*b*) *Irregular* as in some species of Tenthredinidæ and in the Psychidæ.

Under a fourth head *Pædoparthenogenesis*, Henneguy places that rare form of parthenogenesis in which young are produced by the immature insect, which may be either the larva or the pupa. This phenomenon occurs in a few Diptera (Cecidomyidæ and Chironomidæ) and is usually termed Pædogenesis. I prefer that this term should be retained, and as this is an extremely specialised form of parthenogenesis, it should not be classified with the other forms.

It is impossible at the present stage of enquiry to make a satisfactory classification of parthenogenesis from a cytological point of view. Marchal (60) and Loisel have each brought forward classifications, which include those cases found in other groups of animals. The results which we have so far appear to indicate that parthenogenesis is equivalent to internal agamogenesis.

In this summary only those insects in which the maturation of the parthenogenetic ovum has been studied will be considered. The chief feature of these cytological changes which occur is the formation and fate of the polar nuclei.

HYMENOPTERA.

APIDÆ.

Apis mellifica. An excellent account of the progress of our knowledge of the parthenogenesis of this insect and the theories connected with it is given by Phillips (78).

Blochmann (11) first described the polar bodies of the drone-egg. Petrunkewitsch (74) has studied the changes which take place in the maturation of the drone-egg, and in a later paper (75) he gives an account of his investigations as to the fate of the polar nuclei.

The nucleus of the ovum approaches the periphery of the egg. Here it undergoes two successive divisions (*Fig.* 8). In this manner four nuclear masses, groups of chromosomes, are formed. Of these the two outer portions are the halves of the first polar nucleus, and the two inner are the second polar nucleus and the female pronucleus. Each nuclear mass contains eight chromosomes, the original nucleus having contained sixteen. The inner half of the first polar nucleus and the second polar nucleus fuse (*Fig.* 9), and form a single nucleus (*s*) containing sixteen chromosomes. Petrunkewitsch terms this the "copulation-nucleus," an unfortunate term in my opinion, and I propose to call it the *Syntelosome.* The outer half of the first polar nucleus wanders outwards, becomes flattened against the periphery and degenerates; the female pronucleus migrates inwards (*Fig.* 10).

Petrunkewitsch has followed the syntelosome, and finds that it gives rise to the germ cells of the male, by subsequent divisions in the dorsal region of the embryo. The cells thus formed being the primordial germ cells forming the spermatogonia. As these arise from the

original syntelosome they contain sixteen chromosomes. Giglio-Tos (38) has pointed out that these observations of Petrunkewitsch are discordant with those of Meves (64), who has attacked the problem from the other end by studying the spermatogenesis of the drone bee. Unfortunately Meves' description and figures of the chromosomes are not clear enough to permit me to come to a definite opinion, and I do not think that they can be of use in either contradicting or supporting Petrunkewitsch's observations.*

The somatic cells are formed from the female pronucleus. As this only contains half the somatic number of chromosomes, Petrunkewitsch suggests that the normal number is formed by a subsequent mitotic division without a separation of the halved chromosomes.

TENTHREDINIDÆ.

Many species of saw-flies lay eggs which develop parthenogenetically. Taschenberg (93) gives a list of the species in which parthenogenesis is found. Two forms occur—Homoparthenogenesis and Heteroparthenogenesis, and there may be thelyotoky, arrhenotoky or deuterotoky. As a general rule the females are much more numerous than the males. It has also been found that the parthenogenetic young are constitutionally weak and may die before attaining maturity. As either or both sexes may be produced, this constitutional weakness

* Meves (64) finds that the primary spermatocyte contains 16 chromosomes. It gives off a small anucleate bud which he considers to be a rudimentary spermatocyte. The large secondary spermatocyte then divides, forming a small spermatid which he describes as abortive; the large residual spermatid forming a single spermatozoon. The author does not state whether there is any reduction in the number of chromosomes in the last division, and his figures are not clear enough to enable me to judge.

cannot be said to be compensated for by thelyotoky, and, as Sharp has pointed out, it is in the most abundant species in this country, the currant saw-fly, *Nematus ribesii*, that one finds arrhenotoky occurring.

Doncaster (34) has recently investigated the maturation and early development of the parthenogenetic ova of several species of *Nematus* and allied forms.

Nematus ribesii. Doncaster finds that the nuclear changes in the maturation of the parthenogenetic ovum of this species are very similar to those observed by Petrunkewitsch in the drone-egg. There are two successive mitotic divisions of the nucleus (*Figs.* 1-4) which take place in a line at right angles to the surface of the egg. The outer half of the first polar nucleus becomes flattened against the egg membrane; the inner half fuses with the second polar nucleus to form the syntelosome (*Figs.* 5, 6, *s*). The female pronucleus travels inwards and becomes buried in the yolky material. The syntelosome contains either 14 or 16 chromosomes (the author is not quite certain) and remains as a group of isolated chromosomes as far as the blastoderm stage.

Nematus lacteus. (This species is probably arrhenotokous, as it closely resembles *N. pavidus*, which produces males from unfertilised eggs.) The maturation and fate of the polar bodies of the unfertilised egg of this species are very similar to that of *N. ribesii*. There is not, however, a complete fusion of the inner half of the first polar nucleus with the second polar nucleus. The two groups of eight chromosomes lie side by side. One of these soon disappears, the other persisting a little longer. In the thelyotokous species *Poecilosoma luteolum*, *Hemichroa rufa* (which may produce a few males) and *Croesus varus*, there is no conjugation of the inner half of the first polar

nucleus with the second polar nucleus (*Figs.* 11-14). The process of polar nuclei formation is otherwise similar to that of the previous species. They may take up positions farther apart. They move towards the periphery of the egg and in *P. luteolum* degenerate, or the products of the first polar nucleus degenerate and the second becomes resolved into chromosomes. It is also found that the number of the chromosomes in the yolk nuclei, which are derived from the egg-nucleus, remain at the maturation number, as far as can be followed, that is to the formation of the blastoderm. Doncaster does not, with Petrunkewitsch, fall back on the theory of Weismann, that every ovum possesses a power of growth sufficient to double its nuclear substance, but thinks that there is no reduction, as in *Rhodites rosae*. This conclusion is only provisional, and it cannot be definitely decided until the oogenesis and spermatogenesis have been worked out.

Formicidæ.

Lasius niger. The maturation of the unfertilised ovum of this arrhenotokous ant has been studied by Henking (44). He found that three polar nuclei were formed. At one stage there is a fusion of the inner half of the first polar nucleus with the second polar nucleus to form a syntelosome, this stage being very similar to that which he found in the maturation of the fertile ovum (*cf. figs.* 327, 259). Later he observed a dissolution of these nuclei. In a further stage he observed a scattered mass of chromatin bodies, which he considered to have been formed from the polar nuclei, in the peripheral cytoplasm of the egg. Their fate is unknown; their resemblance to the mass of cells resulting from the sub-division of the syntelosome in the bee is striking.

CHALCIDIDÆ.

The embryonic development of some species of parasitic Hymenoptera is very remarkable and extremely interesting. As the unfertilised eggs of the species investigated are able to develop parthenogenetically giving rise to males, they will be considered here.

Encyrtus fuscicollis. This small hymenopteron deposits its eggs in the eggs of the larva of a lepidopteron *Hyponomeuta cognatella*, and probably in other caterpillars. Bugnion (19) first described the development of the larval *Encyrtus* but did not study the embryological changes. This, however, has been done recently by Marchal (60) whose account is most clear, and is accompanied by excellent figures. The first stage of the ovum which he was able to obtain was that in which there were two nuclei. No mention is made of polar bodies as he was unable to obtain earlier stages. The ovum lies in the body cavity of the embryonic caterpillar, and grows rapidly. One of the nuclei, which he calls the paranucleus, grows very large, the others subdividing until a large number are formed. These last collect in groups, the protoplasm surrounding them becomes rounded off and they form separate morulæ. The paranucleus divides up into a large number of parts, which ultimately with their accompanying cytoplasm form an investing sheath round the morulæ. External to this an enclosing membrane is formed from the tissues of the host. By continued growth and development of the morulæ, they displace one another until they are placed end to end in a string. Each of these morulæ forms a separate embryo. They are all enclosed by two investing membranes, an inner which Marchal terms the trophoamnion, which also separates the embryos from each other by transverse septa, and is

derived as shown before from the original paranucleus; and an outer layer of polygonal cells originating from the host. The trophoamnion regulates and controls the food of the embryos which it derives from the blood of the host. In this manner the larvæ grow until they attain a certain size, they then enter and live in the blood of the host, and finally, after devouring the vital parts, pupate in the skin of the full grown caterpillar.

This process of germinogony as observed in *Encyrtus* and also in *Polygnotus minutus* which Marchal also investigated, has been studied in another Chalcid *Litomastix truncatellus* by Silvestri (85).

Silvestri was fortunate enough to study the maturation of the ovum. He found that the maturation of the parthenogenetic ovum is similar to that of the fertile ovum. Three polar nuclei are formed, which fuse later to form a single nucleus. In the segmentation of the ovum, this single 'polar' nucleus becomes segmented off in an apical mass of cytoplasm, in which it divides up into a large number of nuclei. As development proceeds, this cap of cytoplasm containing dividing nuclei surrounds the segmenting germ mass; the original ovum is not mono-embryonic, but gives rise to a large number of embryos. These embryos are male if the ovum is parthenogenetic and female if it has been fertilised, as is found in *Encyrtus* and *Polygnotus* also.

The fate of the polar bodies is very remarkable in *Litomastix*, and if the development of this species be compared with that of *Encyrtus*, it seems probable that the paranucleus of the latter may have arisen from the polar nuclei, though unfortunately Marchal was unable to study the maturation stages. He found the stage with two nuclei, one of which may have been the female pronucleus and the other the 'polar' nucleus.

CYNIPIDÆ.

Rhodites rosae. Thelyotoky and deuterotoky both occur in the gall-flies. Henking (44) has studied the maturation of the unfertilised ovum of *Rhodites rosae.* In this insect the males are rare, but they are occasionally found. Henking found that in the nucleus of the ovarian egg there were only nine chromosomes, the somatic number being 18. The nucleus undergoes two divisions; three polar nuclei and the female pronucleus are formed in this manner, each containing nine chromosomes. In the cleavage nucleus, however, 18 chromosomes were observed. The second polar nucleus and inner half of the first polar nucleus fuse, the outer half of the first polar nucleus going to the periphery. Later the outer half of the first polar nucleus wanders inwards, and fuses with the syntelosome. This takes place about the same time as the female pronucleus begins to divide. As Henking did not examine any later stages he throws no light on the fate of the 'polar' nucleus.

In the maturation divisions of *Rhodites* there is no reduction. The doubling of the chromosomes in the female pronucleus before undergoing division is also interesting. Henking considers that there was a pairing of chromosomes before the first maturation division—the pairs being resolved at the beginning of the cleavage divisions.

DIPTERA.

Pædogenesis occurs in this order of insects in some species of *Miastor*, a genus of the Cecidomyidæ and in *Chironomus.*

Miastor. Metschnikoff (63) studied the development of the unfertilised ovum of this insect. Although he was unable at that time to investigate the minute nuclear

changes, his observations are of interest. In the early stages of the segmentation of the ovum, he describes and figures 'Polzellen.' He traced these and found that they underwent further subdivision and finally formed the genital rudiment. Meinert (62) six years later investigated the development of the same insect. He considered that the 'Polzellen' were separated off from the layer of blastoderm cells, and disbelieves the idea that they wander off later to form the genital fundament, although he apparently did not trace their fate. The 'Polzellen' may not be the true polar bodies as is shown in the next account of *Chironomus*.

Chironomus. Balbiani (1) followed the 'globules polaires' which are formed at the posterior end of the ovum of this insect and traced them to the primitive germ cells. Ritter (81) investigated the early embryonic development of *Chironomus*. He describes the *true* polar nuclei, which apparently have no relation to the 'Polzellen,' they disintegrate before the latter are formed, and lie in the anterior region of the egg. He traced the 'Polzellen' to the genital rudiments, and so confirmed Balbiani's conclusions. Metschnikoff's conclusions as to the fate of the 'Polzellen' in Miastor were probably correct.

From Ritter's observations it appears that the Polzellen are in no way connected with the polar nuclei; they probably arise in the same way in *Miastor*, but the subject needs reinvestigating.

LEPIDOPTERA.

A few cases of Tychoparthenogenesis, which occurs in this group, have been investigated cytologically. Males and females may be produced. The maturation of the

unfertilised eggs has been studied chiefly by Blochmann (8) Platner (79) and Henking (44). In *Liparis dispar** and *Bombyx mori* two polar bodies are formed as in the fertile egg. In many cases the embryo is unable to complete its development. Nussbaum (72) found that only 2% out of 1,100 unfertilised eggs of *B. mori* segmented, but they did not hatch out. Henking obtained an unfertilised ovum of *Leucoma salicis*, but he examined it at too late a stage to observe the formation of the polar nuclei. Tichomiroff (94-99) has caused the partial development of unfertilised ova of *B. mori* by mechanical and chemical stimuli. It is difficult to say whether this is a result of artificial parthenogenesis in every case, as the eggs might have had already the power of developing by natural parthenogenesis.

HEMIPTERA.

APHIDÆ.

Aphis rosae. Blochmann (8) first investigated the maturation of the parthenogenetic ovum of *Aphis*. He found that a single polar body was given off. His observations have been confirmed by the more recent work of Stschelkanovzew (91) and Miss Stevens (86). I have recently studied the maturation of the parthenogenetic ovum of this species and my observations confirm those of Miss Stevens. There is a single mitotic division and one polar body is given off from the egg. It lies at the periphery, but is absorbed later (*Fig.* 7). The number of chromosomes is 10 and there is no reduction in the single maturation division. They occur in five pairs of different sizes. Stschelkanovzew observed 14 chromosomes in one equatorial plate, but I think he counted chromosomes

* *L. dispar* is the destructive 'gipsy moth' and is also called *Ocneria aispar.*

which had been cut as they are often long and curved. After the reabsorption of the polar body, the polar nucleus appears to take no further part in the subsequent cleavage stages. A simple mass of chromatic substance can be followed to a late segmentation stage.

Stschelkanovzew believes that there is a passage of achromatic material from the cytoplasm of the ovum into the egg-nucleus, where it is changed into chromatic substance. It is interesting to note that the oocytes which develop into parthenogenetic ova are different in character and more numerous in the ovary than the fertile ova. They are smaller and very poor in yolk, the difference between the two kinds of ovary being very noticeable, even before any maturation has taken place. This fact is of some importance as showing that the egg is already destined in the ovary of *Aphis* to be either parthenogenetic or fertile. This may be either a state of affairs which has been arrived at through very many generations of parthenogenetic cycles, the phenomenon having been long acquired, or it may be the effect of nutrition upon the reproduction of sex.

ORTHOPTERA.

Parthenogenesis occasionally occurs in this group, and although it has been studied in a general manner, I am unacquainted with any cytological investigations.

COLEOPTERA.

Osborne first recorded the occurrence of parthenogenesis in this order. The only investigation which has been made into the cytological changes is that of Saling (82) on *Tenebrio molitor*. The ovum after some remarkable nuclear changes, which I am inclined to believe were of a pathological nature, failed to segment. Very brief reference is made to the maturation stages.

GENERAL CONSIDERATIONS.

In studying the work which has been done on parthenogenesis in insects, certain points of similarity occurring in the changes undergone by the nucleoplasm and cytoplasm of the ovum are noticeable. In the majority of forms in which the maturation of the fertile ovum has been studied, it is found that true polar bodies are formed; and these may be either separated completely from the ovum or lodged in a depression. In other cases the two polar nuclei, or sometimes only one, may be contained in a cytoplasmic projection at the periphery of the ovum. Except in the case of *Aphis* and the tychoparthenogenetic ova of Lepidoptera which may, perhaps, be left out of consideration on account of their exceptional occurrence, and the usual failure of the resulting embryos to complete their development, we do not find any definite polar bodies formed in the parthenogenetic ova of insects. Three polar nuclei are formed, but they are not extruded as polar bodies.* This appears to me to have some significance as regards the power which the ovum possesses of developing parthenogenetically.

The two results of fertilisation are Amphimixis and Embryogenesis. In parthenogenesis the former is excluded, and only the latter takes place. The question then arises—what is the factor in the parthenogenetic egg which replaces the stimulus to development which is brought about by the entrance of the spermatozoon?

The statement of Boveri (12) that 'das Centrosoma ist das eigentliche Theilungsorgan der Zelle: es vermittelt die Kern- und Zelltheilung' is certainly disproved by

* In the fertile ova of a few insects it is found that the polar nuclei are not extruded, but remain at the periphery of the egg

parthenogenesis, as there is no centrosome brought in by a spermatozoon, and in many cases the centrosomes do not appear until the segmentation stages; we cannot, therefore, look to the centrosome as the cause of Embryogenesis.

The suppression of the second polar body in *Aphis*, and as Brauer (18) found in *Artemia*, its fusion with the egg nucleus, seemed at one time to support Boveri's (13, p. 73) suggestion that parthenogenesis is the result of fertilisation by the second polar body, that is, the second polar nucleus took the place of the sperm nucleus. This suggestion, however, receives no support from the majority of cases of parthenogenesis in insects, in which there is no fusion of the second polar nucleus with the egg nucleus.

The complete suppression of definite polar body formation, or at least of the formation of the second polar body may explain the ability of parthenogenetic eggs to develop. There may be a cytoplasmic fertilisation, so to speak. The evidence we have, seems to me to suggest that there is a chemico-physical relationship between the nucleoplasm and cytoplasm, which is necessary for the initiation of embryogenesis, and which is brought about by the entrance of the spermatozoon. The non-extrusion of polar bodies may bring about this relationship. What the exact nature of this relationship is, or what are the causes which operate, it is impossible at present to say. It can be brought about by artificial means as Loeb (58-59) Delage (23-27) Petrunkewitsch (77) Morgan (66-69) and others have shown and differences of osmotic pressure, ferments, metallic ions have all been suggested as possible causes, but the problem is still unsolved.

As Morgan (70) has recently pointed out little attention is paid to the *rôle* of the cytoplasm. The work of Delage (29) in Merogony, in which he fertilised portions

of eggs not containing a nucleus and obtained embryos from the same disproves the idea that fertilisation is an affair of the nuclei only.

NATURE OF PARTHENOGENESIS.

Parthenogenesis may be regarded as internal agamogenesis. But it is not by any means of the same nature throughout the group. The polar bodies are now generally regarded, I think, as abortive ova. The experiment of Francotte (36) who was able to fertilise a rather large polar body was satisfactory proof of the fact. It follows then, that if we regard the polar nuclei as nuclei of abortive ova, or better abortive gametes, there are different kinds of individuals produced in the different types of parthenogenesis which we find occurring.

In *Aphis* the formation of a second polar body is suppressed, the resulting individual is then developed from a cell the nucleus of which may be taken to represent the fusion of two sister gametes. In *Bombyx mori* and *Liparis dispar* where two polar bodies are formed the individual is a monont,* having developed from a single gamete. In only one case has the complete development of a parthenogenetic ovum, which does not extrude polar bodies, been followed, namely, that of the bee. In this form the second polar nucleus fuses with the inner half of the first polar nucleus, and from the syntelosome so formed, the male germ cells develop. These germ cells develop then, from a nucleus which is really a diont, the result of the fusion of two (abortive) germ nuclei of the relation of cousins. The somatic cells of the bee are derived from the egg nucleus. Consequently the drone bee is a compound structure, as Blackman has also pointed out. The germ cells have a different origin from the

* I am indebted to Dr. Sharp for this word.

somatic host which lodges them. This fact is interesting in the light of Gaskell's (37) recent paper in which he regards the adult individual as a neural syncytium holding in its meshes the germ cells, and others, unconnected with the nervous system. Petrunkewitsch's observations require confirmation before they can be finally accepted.

The fate of the polar bodies has not been traced in any other cases except by Silvestri in *Litomastix* where they develop into an embryonic investment, which I suggested might be the fate of the polar bodies of *Encyrtus* and *Polygnotus*. In the majority of cases they simply become resolved into a number of chromosomes and disintegrate.

Determination of Sex.

The desire on the part of some writers to treat the sex character as being of the same category as somatic characters, which include secondary sexual characters, seems to me to be quite unjustifiable. Sex has a much more fundamental significance than have somatic characters. In the sexes themselves, the essential difference between the male and female consists in the fact that they produce different kinds of reproductive cells. All other differences—that is, somatic differences, are secondary and subservient to the act of reproduction and the development of the individual.

At the present stage of our knowledge, I think that Castle's (21) attempt to fit in the facts of maturation with the Mendelian principles of dominance and segregation is premature. Comparatively few cases of parthenogenesis in insects have been worked out with a view to studying the maturation of the ova, but of the few cases which we already know, a fairly large percentage would

have to be considered by Castle as exceptions to his view (21, p. 200) that 'a segregation of sex characters takes place at the formation of the second polar cell,' and later, 'hence, if the egg which has formed two polar cells develops without fertilisation, it must develop into a male.' The two exceptions which he considers are *Rhodites rosae* and *Bombyx mori*. As has already been stated, very few males are produced by the parthenogenetic eggs of *R. rosae*. There is no reduction in the number of chromosomes, so that there is no segregation of sex characters. Castle admits this, and suggests that the egg retains a capacity to eliminate the dominant female character, which it does occasionally, and so males are produced, as in other parthenogenetic animals, ***under appropriate conditions***. (The italics are mine.) He finds it necessary to bring in other conditions than the mere segregation of male and female characters in the maturation divisions. His treatment of the second exception, that of *B. mori* and *L. dispar* is still less convincing. In the parthenogenetic eggs of these moths two polar bodies are given off, as in the normal fertile egg, and the small proportion of these eggs which develop into perfect insects are of both sexes. In attempting to explain this exception, he says (p. 205) 'But it is entirely possible that in the very exceptional egg which develops normally, a second maturation division has for some reason failed to take place, or after it has taken place, a reunion has occurred of the second polar nucleus with the egg nucleus, as sometimes in the egg of *Artemia*, according to Brauer. Such a reunion would bring together again the sex characters segregated in maturation, and would produce the physiological and morphological equivalent of the cleavage nucleus of a fertilised egg. A similar result would follow the complete suppression of the second

maturation division.' I do not consider the suggestion drawn for a comparison with *Artemia* a satisfactory explanation. The parthenogenesis of *Artemia* is highly specialised, is the chief mode of reproduction and is probably of old acquisition, whereas both *B. mori* and *L. dispar* only occasionally reproduce parthenogenetically and the nuclear changes in the maturation of their parthenogenetic eggs do not differ essentially from those of the fertile egg. There does not appear to be any tendency for either a suppression of the second polar nucleus or a fusion of the latter with the egg nucleus.

Petrunkewitsch (76) believes that Brauer's second type of maturation of the parthenogenetic ovum of *Artemia*, where there was a fusion of the second polar nucleus with the egg nucleus, was pathological, as he searched specially for it in material from the same locality and failed to find it. Castle thinks that Petrunkewitsch investigated the winter egg and that the second type of maturation occurs in the summer egg, although Brauer himself does not mention the fact. He cannot, however, bring forward any such reason as this for Henking's and Platner's failure to observe a similar phenomenon in the eggs of *B. mori* and *L. dispar*.

But now there are some more exceptions which Castle's theory would not be able to explain, namely the thelyotokous saw-flies such as *Parcilosoma luteolum* and others which Doncaster has investigated. In these, the number of the chromosomes of the maturation divisions remains the same and there is no fusion of the nuclei; the polar nuclei apparently disintegrate. These exceptions to Castle's theory are sufficient, I think, to show that it fails to explain the problem of sex in the parthenogenesis of insects. They are not insignificant in number but form a fair proportion of the cases which have been studied

cytologically up to the present time. I am also unable to fit in the facts which we find in *Aphis* with this theory, Miss Stevens too, appears to find it difficult. In referring to Castle's theory, Bateson (5, p. 127) says 'While admitting the likelihood of this suggestion, we feel that for the present it should be received with caution. In particular, we doubt the conclusion that *both* ova and spermatozoa (after a reduction division) are always bearers of either the male or the female character. It seems more likely that special cases will present special phenomena in this respect.'

Doncaster (34) in attempting to explain the phenomena which he observed in the maturation of the unfertilised eggs of the saw-flies, combines Castle's hypothesis of the separation of male and female bearing nuclei with that of Le Dantec, who considers that maleness and femaleness are similar to molecular forces which cause an attraction between bodies bearing them, comparable to bodies charged with opposite kinds of electricity. In this manner he explains the fusion of the inner polar nuclei in the arrhenotokous forms by supposing that the egg nucleus is ♂ and the three polar nuclei ♀, ♂ and ♀ respectively, proceeding outwards, so that there will be an attraction between the two inner nuclei. In the thelyotokous forms the egg nucleus is ♀, and the polar nuclei ♂, ♂ and ♀ respectively, there will be no fusion then of the inner polar nuclei. His idea is very ingenious, but requires further testing, as he admits.

Another recent theory of sex is that of Ziegler (106) which is based on the two assumptions that sex is a character which can be transmitted by inheritance and that the hereditary characters reside in the chromosomes. The fact that Ziegler states that he knows that 'diese Erklärung der Entstehung des Geschlechts nicht für alle

Tiere zutreffend sein kann. Die eigentümlichen Fortpflanzungsverhältnisse der Honigbiene, der Gallwespen, der Daphniden, der Rotatorien, des Dinophilus, u.s.w. lassen sich nicht in dieser Weise aufklaren,' is of itself a sufficient reason for not accepting it, were it not otherwise unsatisfactory, as Morgan (71) has shown.

It is not by excepting these 'peculiar' cases of parthenogenesis, but by studying them, that we shall have more light thrown upon the problem of sex. The solution of the problem of the determination of sex is still far off, and will continue so until we have more evidence than is available at the present time.

The view that nutrition is an important factor in the determination of sex has received some support. Siebold (84) first showed that in the saw-fly (*Nematus ventricosus*) more females than males were produced when abundant nutrition was available. Maupas (61) was able to control the sexes of *Hydatina* by feeding. Other evidence is afforded by Nussbaum on *Hydatina*. Hoffmann (52) from his study of plants came to the conclusion that the males were incompletely developed individuals, formed under unfavourable conditions. The Lepidoptera have furnished material for many experiments with a view to finding out the effect of nutrition on sex. Mrs. Treat (100) stated that males and females were formed according as the caterpillars were poorly or well-fed. Cuénot (22), however, was unable to come to this conclusion from his experiments, and Kellog and Bell (53) experimenting on the same insects failed to obtain evidence in support of it. The difficulties in investigating the effect of food on caterpillars are considerable, the chief being that the female imagines are larger than the males, and consequently require more food during their larval stage, any diminution then in the quantity of the food will affect the

female larvæ first, and so by their deaths the percentage of males produced will be increased. Strasburger (87) for plants, Beard (7) and Lenhossek (57) all support the idea on theoretical grounds that sex is already present in the germ.

Absence of fertilisation, that is, development by parthenogenesis, may produce different sexes in closely allied forms, as in the saw-flies and in other Hymenoptera. It is very probable that in the ovum we have only one sex; this may differ in allied forms, but it is characteristic of the species. The production of the opposite sex is determined by other factors, chief of which is fertilisation. What other factors influence sex remains to be determined. I believe that nutrition does in some forms influence the production of sex, for I fail to see otherwise how a sexual generation is produced in the Aphides, as shown by Kyber (55), by the diminution of sap in their food, caused by the tissues becoming woody, in the summer months, that is when everything which man can observe is favourable for the continued production of parthenogenetic females, except nutrition.

THE CHROMOSOMES.

The facts which we have at hand relating to the number, division, and *rôle* of these bodies in cytological phenomena are almost beyond comprehension. That the chromosomes should have attracted so much attention is not surprising, as they are the chief visible factors of cell division and reproduction which can be dealt with. The rapid progress which has been made in the study of these bodies has been due very largely to the fact that they are considered to be the bearers of hereditary characters, so it has been thought that by a study of

their changes some explanation might be found for the problems of heredity and sex.

It may be of some interest to examine the evidence we have for considering the chromosomes the *sole* bearers of hereditary characters.

In 1866, Haeckel ('43, p. 288) wrote 'wir werden den Kern der Zellen als das hauptsachliche Organ der Vererbung, das Plasma als das hauptsachliche Organ der Anpassung betrachten konnen." When the chromosomes were observed we find Hertwig (47), Strasburger (87, 88), and Weismann (104) considering them as the bearers of the 'Vererbungssubstanz.' It was, however, considered by many to be a proven fact that the chromosomes were the bearers of the hereditary characters when Boveri (14) made known his experiments on fertilising fragments of Echinoderm ova.* He shook up echinoderm eggs in a tube half filled with sea-water for some time. After this rough treatment, he poured spermatozoa of another species into the liquid containing whole eggs and fragments. In this manner he obtained Plutei, concerning which he made statements to the effect that there were a number of small specimens, which he concluded came from fragments of eggs; as the nuclei of these same plutei were very small, he concluded they came from enucleated fragments; also they bore pure paternal characters. Therefore he concluded that the cytoplasm of the ovum does not transmit any maternal characters; it followed then, that the nucleus alone was the bearer of the hereditary characters. From this experiment it certainly appeared as if the nucleus were the only part of the cell concerned in the transmission of the hereditary characters.

When these experiments were repeated by Verworn

* O. and R. Hertwig (48) first attempted fertilising fragments of Echinoderm ova. But their experiments were not conclusive.

(101), Morgan (67), and Seeliger (83), and more recently by Delage (28, 29, 30), their observations flatly contradicted those of Boveri. The Plutei which they obtained by fertilising enucleated portions of eggs with spermatozoa of a different species, did not bear wholly paternal characters, but maternal ones also. They found that the nuclei vary in size; the nucleus is often below the normal size in embryos, having small cells coming from nucleated portions of ova, consequently it is not at all certain that the nuclei were absent in the fragments which gave rise to Boveri's plutei. Further, the hybrid plutei which develop from whole fertilised ova are very variable, their characters are not necessarily intermediate between those of the parent species; some may be intermediate, but there are others having characters wholly paternal.

These observers performed their experiments much more carefully than Boveri, who did his very roughly, and did not demonstrate the absence or presence of the nuclei in the fragments. Nor did he obtain his enucleated portions of ova, if there were any [there is no proof that there were], by cutting them up individually. The results of Delage and others are much more trustworthy. But in spite of the fact that Boveri's experiments have been contradicted, though he has attempted to defend his views (16), a great amount of faith is placed in them. The tendency is to take it as a proven fact, and to argue on the assumption, that the chromosomes are the sole bearers of hereditary characters, which I believe to be far from proven.

Working on this assumption, Castle (21), Sutton (92), Häcker (42), Montgomery (65), and others have suggested hypotheses to account for and explain the nature of heredity and sex. Each worker has attempted to make the chromosome data fit in with the theory which

he champions, with a certain amount of success one must admit. But it certainly appears to be pushing the matter beyond reasonable limits when Boveri and other investigators suggest that different sized chromosomes may bear different or correlated characters.

The cytoplasm of the ovum and the spermatozoon is left out of the question entirely in questions of heredity by these investigators. This is an unjustifiable proceeding. The cytoplasm of the spermatozoon is in a highly concentrated condition, as also is the nucleus, and we have no evidence against the view that the cytoplasm of both ovum and spermatozoon or of either of them may bear some hereditary characters as well as the nucleoplasm. It must be remembered also, as will be shown later, that in some ova a large quantity of nucleoplasm is scattered in the cytoplasm. Until it has been proved that the cytoplasm takes no part whatever in the transference of hereditary characters, all hypotheses which only take the chromosomes into account, must be received with due caution.

In those parthenogenetic eggs of insects in which there is a reduction in the number of chromosomes, it is probable that the normal number is formed later as a physiological necessity. I believe, with Delage, that the cell is able to re-establish the normal number of chromosomes, and that 'si le nombre des chromosomes est constant chez les animaux, ce n'est pas, comme on le croit, parceque ces organites ont une personalité qui les rend individuellement permanents, c'est parceque ce nombre est une propriété specifique de la cellule, une constante de la cellule.' This raises the question as to whether a fixed number of chromosomes always occurs. It is usually considered that this is the rule, but certain observers have found that this is not always the case.

Von Winiwarter (105) has found that the number is very variable in the rabbit, varying from 36 to 80, the average being about 42. The reduced number in the sexual cells of the rabbit is 12, according to this, then, the somatic number should be 24. Guignard (41) has shown that in certain plants the chromosomes may remain at their reduction number in the somatic cells. Farmer and Shove (35) have shown that in the somatic cells of *Tradescantia* the chromosomes vary in number from 26 to 33. There are two varieties of *Ascaris*, *univalens* and *bivalens*, and according to Brauer two types of parthenogenetically developed *Artemiæ*, possessing 84 and 168 chromosomes respectively. Delage (32, p. 127), who also calls attention to this fact, found in his experiments on fertilising enucleated portions of the ova of *Strongylocentrotus*, that the cells of the developing embryo contained 18 chromosomes, although the spermatozoon only contained nine, the reduced number. He also found 18 in the cells of the embryos of the same species which had been made to develop parthenogenetically. All these facts tend to make one believe that there is not an absolute permanency in the number of chromosomes, and favour Delage's view that the number of chromosomes is 'une propriété cellulaire.'

The suggestion of Stschelkanovzew's (91) raises the further question, to what extent do we get chromatin formed by the change of achromatic material which has entered the nucleus from the cytoplasm? It is extremely probable that this takes place to a greater extent than is recognised. Cameron (20) has found in studying the development of nerve cells that there is a chromatisation of achromatic material, which he considers as chromatin in a nascent condition. The presence, in some cases, as *Alcyonium* and other Cœlenterates, as shown by Hickson, of a large quantity of chromatic material in the cytoplasm

supports such an idea. But our knowledge of the microchemistry of the cell is still too meagre to arrive at absolute certainty on this and other points.

The usual preconceived idea of the unchangeable nature of the nucleus does not rest upon a solid foundation. As Gruber (40) in the Protozoa, Hickson (50), Hill (51), and Hargitt in the Cœlenterates, Henking in Insects, and many others in other groups, have all shown that the nucleus in many cases loses its compact character and fragments, nor is this a pathological condition, but quite normal. In some cases the chromatin granules remain clustered together, in others they become scattered through the cytoplasm of the ovum, either remaining in that condition, or collecting later into definite nuclei. These facts do not support any theory of the individuality of chromosomes, as put forward by Rabl (80), Baumgartner (6), Boveri and others. Nor do they furnish evidence for any hypothesis which considers the chromosomes *only* as the bearers of hereditary characters.

The evidence available on the cytology of parthenogenesis in insects is too small and too diversified at present to allow us to draw any conclusions on questions of heredity and sex, about which we know little. It is certain, however, that the accumulation of observations and facts on this subject and not of hypotheses and theories on insufficient facts, will be the only way in which we shall be able to elucidate those, at present, unsolved riddles.

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EXPLANATION OF PLATES.

e. Egg nucleus. *p*1. Outer half of first polar nucleus. *p'*1. Inner half of first polar nucleus. *p*2. Second polar nucleus. *Pb*. Polar body. *S*. Syntelosome (= *p'*1 + *p*2).

Plate I.

1. Formation of polar nuclei in parthenogenetic egg of *Nematus ribesii* (diagrammatic after Doncaster).

Fig. 1. First polar spindle.

Fig. 2. First polar spindle dividing into the two polar spindles of the second polar mitosis.

Fig. 3. Second polar mitosis.

Fig. 4. Egg nucleus (*e*) and three polar nuclei.

Fig. 5. Fusion of two inner polar nuclei to form the syntelosome.

Fig. 6. Resolution of syntelosome into 16 chromosomes.

2. *Aphis rosae.*

Fig. 7. Two mature parthenogenetic ova of *Aphis rosae* in their follicles. The single polar body of the upper ovum lying at the periphery; the polar body of the lower ovum has been absorbed: × 1500 (drawn with camera lucida).

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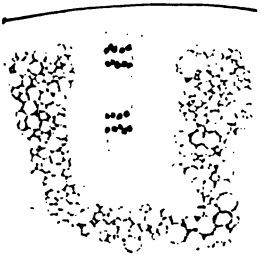

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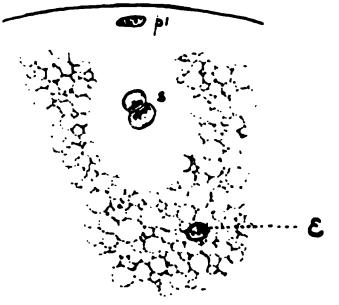

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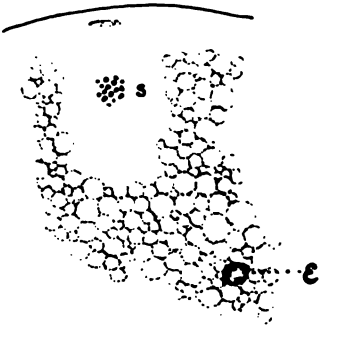

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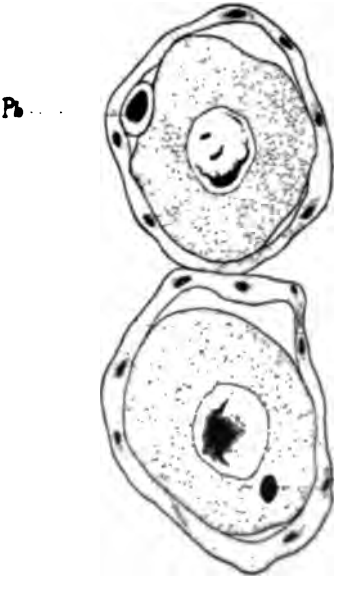


Plate II.

1. Formation of polar nuclei in parthenogenetic egg of *Apis* (after Petrunkewitsch).

Fig. 8. Second polar mitosis, each of the three polar nuclei and the egg nucleus contain 8 chromosomes, the reduced number.

Fig. 9. Fusion of the inner half of the first polar nucleus and the second polar nucleus to form the syntelosome (Petrunkewitsch's "conjugation-nucleus").

Fig. 10. The degeneration of the first polar nucleus, and wandering inwards of the egg nucleus. The syntelosome contains 16 chromosomes.

2. Formation of polar nuclei in parthenogenetic egg of *Poecilosoma luteolum* (diagrammatic after Doncaster).

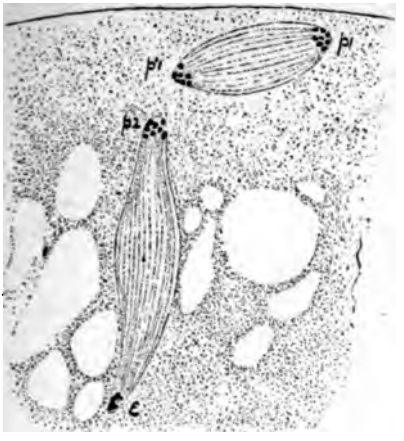
Fig. 11. First polar spindle.

Fig. 12. Second polar mitosis.

Fig. 13. Egg nucleus and three polar nuclei: inner largest, outer smallest.

Fig. 14. Degeneration of polar nuclei and inward migration of egg nucleus (*e*).

8.



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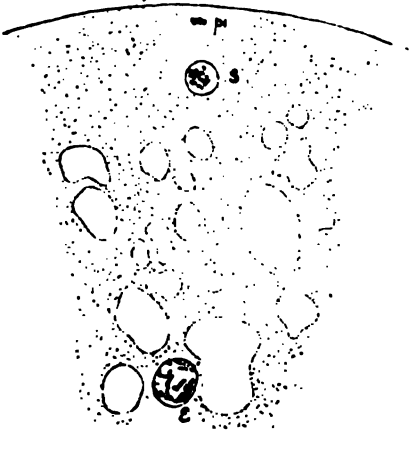

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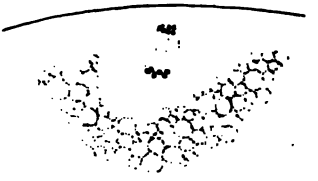

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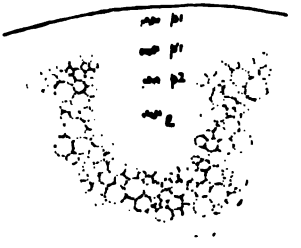

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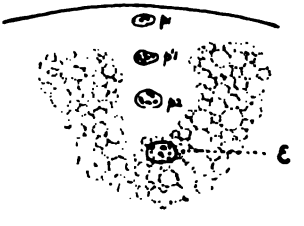

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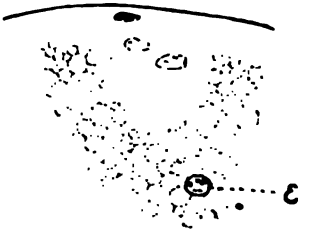

Manchester Memoirs, Vol. l. (1906), *No.* 7.

THE WILDE LECTURE.

VII. "Total Solar Eclipses."

By Professor H. H. TURNER, D.Sc., F.R.S.

Delivered March 20th, 1906.

To those who have never seen a total eclipse of the sun, but have seen partial eclipses, the importance attached to the former may seem somewhat puzzling. A partial eclipse is an interesting but not a very striking phenomenon: we watch the queerly-shaped disc of the sun through smoked glass (or by reflection from water) and we may have seen the crescent-shaped patches of light which filter through the trees; but there is not much suggestion of an astronomical opportunity differing essentially in character from that afforded by full daylight. Those, however, who have been fortunate enough to see the sun completely hidden by the moon's disc know that, when the hiding is complete, there flashes into view a spectacle entirely strange, a wonderful halo or glory of light of a diffuse character which has been called the Corona. Close to the sun this light is very bright—not by any means so bright as a portion of the ordinary sun, but still brilliant compared with the outer Corona. Often there are jets of especially brilliant light of rosy colour, and this colour may be traced, close to the black central disc of the moon, for some distance round the circumference. On the Chromosphere, as this close envelope is called, an important observation was

May 26th, 1906.

made in 1868, independently by two observers, Janssen and Lockyer. They found that its spectrum was made up of distinct bright lines—special and very definite colours, that is to say—and by making use of this knowledge it has since been found possible to see this inner envelope in full daylight without a total eclipse. More recently a new instrument, the spectro-heliograph, has been invented which enables us, following out this same principle, to *photograph* the Chromosphere, as well as other portions of the sun's surface, of which we had practically no knowledge a few years ago. But although many attempts have been made to detect in similar ways the Corona or any part of it, they have up to the present resulted in failure, and a total eclipse remains unique as an occasion for studying a most important part of the sun, viz., all that out-lying portion of many times his own diameter which we call the Corona.

It must not be thought, however, that partial eclipses are of no value; they afford us opportunities for very accurate measures of the relative places of the sun and moon, and this at a time when we ordinarily cannot well observe the place of the moon; for when the moon is nearly "new," it is during the daytime lost in the sun's glare, and near sunrise or sunset is so low in the sky that other troubles arise. Even without accurate measurements (such as we can now make, but which thousands of years ago were undreamt of), a mere record that an eclipse took place is sufficient, if trustworthy, to give us valuable information as to the motion of the moon, or of the earth in its orbit round the sun. There have indeed been found difficulties in interpreting many of these old records, difficulties so great that of late years it has been doubted whether the records are trustworthy enough for this purpose. Ideas of accuracy have no doubt changed in the thousands of

years since the first crude attempts were made to write history, and early historians have been suspected of introducing eclipses into their narratives as scenic effects, much as a painter might introduce into a picture figures or clouds which might have been, but were not, actually there. Within the last year, however, Mr. Cowell, of the Greenwich Observatory, has thrown a new light upon some of these old records. He has shown* that by a new and simple supposition no less than five important records of eclipses, which it had been previously considered difficult if not impossible to reconcile with what we know of the moon's motion from modern observations, could all be brought into line. His hypothesis is indeed a little startling, being no less than a gradual quickening of the velocity of the earth in its revolution round the sun, which we have been accustomed to regard as having been constant for ages, if we disregard variations of a periodical character. But there is more than one possible *vera causa* for such a change of motion. We have long been familiar with such a change in the case of the moon, and indeed there has been a controversy of no mean order about this same "secular acceleration," a controversy which is somewhat of a curiosity,† for it centred round a purely mathematical calculation, about which it might have been supposed there could be no doubt. But in the case of the moon there is no difficulty in assigning the causes for this change, whereas in the case of the newly discovered change in the earth's motion difficulties do arise. We may suppose, for instance, that there is a resisting medium of an extremely tenuous character, and this would produce the proper effect upon the earth; but it would be natural

* See *Mon. Not. Roy. Astron. Soc.*, vol. 65, pp. 861, 867; vol. 66, pp. 3, 5, 35, 36.

† See, for instance, *Mon. Not. Roy. Astron. Soc.*, vol. 53, p. 198.

to suppose that the planet Mercury, which is nearer the sun and therefore moves more rapidly, would be affected at least as much as the earth. Mr. Cowell has accordingly tested* this hypothesis by analysing the observations of transits of Mercury, and found confirmation of the change in the earth's motion, but no evidence of change in that of Mercury. We have learnt, however, in Astronomy to be patient in expecting the interpretation of results so long as we can make sure of their reality from observation, and we may await therefore with patience, although with great interest, the unravelling of this new puzzle which Mr. Cowell's preliminary work has introduced to us.

For the present it is enough to remark that although this work lies in a region where partial eclipses are not by any means useless, nevertheless even here the superiority of total eclipses is manifest. It is one of the points made by Mr. Cowell that the records are much safer to interpret when there is some reference to the appearance of the Corona, and we are able to infer that the eclipse was total. In modern times, also, observations of position are certainly rendered more complete on the occasions of total eclipses, although it is not often that advantage can be taken of this fact, chiefly on account of the more pressing need for spectroscopic and other observations on the nature of the Corona. In 1887, however, the late Otto Struve organised an extensive programme of observations of this kind, hoping to measure very exactly not only the relative places but the relative sizes of the sun and moon. Unfortunately, cloudy weather prevented observations of that eclipse, and not only Struve's assistants but some forty other European Astronomers from all nations returned home disappointed. In 1898 the Astronomer Royal carried out in India a programme of

* *Mon. Not. Roy. Astron. Soc.*, vol. 60, p. 36.

observations for position, with the efficient aid of the Royal Engineers, and last year, one of the prominent members of this Society, Mr. Stromeyer, made some ingenious suggestions for work of this kind at the eclipse which was to take place on August 30th. He had in view not only the determination of places of the sun and moon, but the measurement of distances on the earth's surface, if the observations could be made at more than one place I had the pleasure of discussing the possibilities with him, and at one time I hoped that something might be done at any rate to make the preliminary trial of his ideas, which might encourage more complete application of them in the future. But when the time arrived, and the difficulties of work in Egypt were fully realised, it became clear that any such attempt must be dropped unless we could afford to sacrifice observations demanded by considerations of continuity.

Total eclipses have, however, one great disadvantage—their extreme rarity. The spot on the earth from which the sun is entirely obscured from the moon is a very small one. If we cover up the sun's disc with, say, a threepenny bit held at arm's length, we can only cover it up for one eye; if we open the other eye the sun is not obscured from it, as we become painfully aware. And so though one spot on the earth may be favoured by a total eclipse, another at a little distance is not. The favoured spot does not remain stationary on the earth owing to the motion of the moon between us and the sun, and also to the rotation of the earth. It travels over the surface; but even so the track made is narrow compared with the wide area over which the partial eclipse is visible. Moreover the track has a wayward habit of wandering across regions which are either inaccessible or occupied entirely by ocean; and although we could in the latter case enjoy the spectacle

from a ship, unless we can set up instruments firmly on land the observations made are of little use. In 1904 an eclipse track crossed the Pacific Ocean, skilfully avoiding contact at any point with even one of the numerous islands which seem to have been almost purposely scattered about to entrap it. In January, 1908, a similar track will meet with less success, for we learnt a few days ago that although it will lie for nearly the whole of its length over deep sea, it does cross two small islands, both uninhabited, and both devoted to the growing of cocoa-nuts for the purpose of making Sunlight soap. Some of us are hoping that Messrs. Lever Bros. may see an opportunity of combining business interests with scientific enterprise, by sending out an expedition to observe the eclipse of the sunlight on their property. It is nearly a quarter of a century since a neighbouring island, Caroline Island, was used for such a purpose, but meantime other journeys almost as long, and to places nearly as inaccessible, have been undertaken. In 1896 I had a most delightful expedition to Japan, in company with the Astronomer Royal and Major Hills. We saw many most interesting sights on the way there and back, which lay across America. For instance, we were present at almost the foundation of the great Yerkes Observatory, near Chicago, which has since become famous in astronomical history. We were escorted to our station by a portion of Her late Majesty's China Fleet, and dined on the Flagship with the Admiral; and under the able and hospitable guidance of Sir Ernest Satow we were able to admire the skill and charm of that wonderful people who had just completed their war with China, and have since shown themselves more than worthy foes of one of the great European powers. But unfortunately, delightful though the whole expedition was, the central object of it failed. On the day of the eclipse

there was a dense fog which rendered all our long and arduous preparations futile. In the same year another party had what may be called an inverse experience. After we had left England for Japan, and after other observers had set out for Norway, Sir George Baden Powell took in his private yacht Mr. Shackleton and the late Mr. Stone to Nova Zembla. They narrowly escaped shipwreck, running on a rock not marked on the chart, so that for some days the ship's deck lay at an angle of 45°; when ultimately they got off and effected a landing, the weather was atrocious and hampered their preparations; but the day of the eclipse was fine, and they got excellent photographs, one of which (taken by Mr. Shackleton) of what is now called the "flash-spectrum," marks a definite epoch in such work. Sometimes Fate is cruel enough not only to inflict hardship, but also to rob the observers of the compensation of a fine eclipse. Last year the Labrador expeditions were particularly unfortunate in this way. Mr. and Mrs. Maunder have told us how on arrival they found their intended station occupied by some Indians, who had caught measles, and wished to be near a white doctor; and how in order to reach the next clearing, nearly a mile away, it was necessary (in default of any kind of labour or horses) to wheel thither all their instruments in wheel-barrows, during a protracted shower of rain lasting some days; and how, after they had braved all these discomforts and hardships, they were rewarded by a hopelessly cloudy day for the eclipse. And we must not forget that these expeditions are not only liable to disappointment and fraught with hardship, but are sometimes accompanied by positive danger. In 1889 Father Perry, of Stonyhurst, who, although always a martyr to seasickness, never shirked the longest expedition in the cause of science, bravely started to occupy a station in a malarious

region near the French convict settlement at Cayenne. Perhaps if it had been known beforehand how bad the conditions really were the expedition might have been abandoned, but this was not realised until too late. He fell ill with dysentery, and although he successfully completed the work for which he was sent out, he shortly afterwards died at sea. He was ill on the actual day of the eclipse—so ill that when the observations were completed, and he called for three cheers for the success, he was unable to join in them himself. "I can't cheer," he said, "but I will wave my helmet." It is a memory worth preserving,—that a brave swimmer caught by the cruel waters of disease should go down waving his helmet for the success which had cost him his life.

So great a disaster is fortunately rare in scientific work; but hardship must often be faced, and disappointment is always risked, on eclipse expeditions, which have for their object the best utilisation of the few precious moments during which the corona is visible. It might be difficult to justify this enthusiasm from the cold, logical standpoint of actual knowledge: we do not *know* that the Corona is the most important part of the sun; indeed from what we know up to the present, it may be comparatively unimportant. But the other extreme is also possible; the study of the Corona may give us an invaluable clue for interpreting other solar phenomena, and this very uncertainty is in itself attractive. In any case it scarcely needs explanation that a phenomenon which can only be observed for a few hours altogether in a whole century, should attract more than its strictly logical share of attention. At any one place a total eclipse lasts a few minutes only, say two minutes on the average. Now there are twelve "families" of total eclipses which recur in a regular cycle every 18 years, $11\frac{1}{3}$ (or $12\frac{1}{3}$, according to the occurrence

of leap years) days. The one-third of a day is important, because it moves the eclipse track on the earth's surface just one-third round the earth at each recurrence; so that for the third recurrence (that is, after 54 years, 1 month, and a few days) the track comes back nearly to the same position. Nearly, but not quite, since all these figures are approximate only, and not exact. The track is moved a little north (or a little south) every 54 years, and ultimately disappears from the earth at the north (or south) pole. But to replace any family which dies in this way, another one is born, so that the total number recurring in the 18 years is always about twelve. A new family is to be born at the North Pole in 1909, which is of special interest to us, because at its second recurrence (on June 29, 1927) the track crosses the North of England; and for the first time since 1724 we shall be able to see a total eclipse without leaving England. The track is a little unkind to Manchester, which is left just outside; so that it will be necessary for this Society to make an eclipse expedition to Liverpool. The duration of totality is also very short—only 25 seconds—but the occasion can nevertheless be recommended to everyone as one of remarkable interest, and there is plenty of time for leisurely preparations.

The observations to be made at a total eclipse vary greatly in character. We may direct attention to the parts of the corona lying near the sun, or to the fainter parts at a distance: we may study its form, and its possible changes of form; or its spectrum, and variations in spectrum; or the polarisation of the light; or we may leave the corona altogether and look for planets close to the sun, for seeing which a total eclipse affords a unique opportunity just as for the corona. One modern feature of all such observations may be emphasized—they are nearly always photographic. It is easier and safer to

take proper photographs and study them at leisure than to attempt visual observations in the limited time and under the stress of excitement.

I shall not attempt to give even a brief description of all these observations; but shall, with your permission, select one special point for consideration from a line of investigation to which my own work at recent eclipses has been directed. My special object will be to shew how a *preconceived notion* helps us in studying natural phenomena. The point is not a new one—it is as old as science itself; but every new illustration of it comes with a certain freshness. A beautiful illustration was given nearly 30 years ago by Professor Schuster in almost exactly the same domain as that of which I am about to speak. He attacked * the problem of the distribution of particles round the sun which might, either by scattering the sun's light, or by themselves becoming incandescent, give rise to an appearance such as the corona, and wrote thus:—

"Our problem is an inverse one, and seems at first sight very hopeless. From the observed polarisation of light we are to find out what part of it is due to scattering particles, and, as will be seen, we cannot do this without finding out at the same time in what way the scattering particles are distributed round the Sun, and in what way the light due to other causes varies with the distance from the Sun. I began the calculation in the hope of getting a rough idea only of the amount of polarisation which we might expect. But it appeared that even such observations as we can make during the short time available during a total solar eclipse may yield most important information as to the constitution of the solar corona. I shall shew that combined measurements (*a*) of the

* *Mon. Not. R. y. Astron. Soc.*, vol. 40, p. 35.

polarisation at different distances from the Sun, and (*b*) of the decrease in intensity of the total light of the corona with increasing distance from the Sun will be sufficient to determine all our unknown quantities. Even if such measurements are incomplete, we may gain a rough idea of these quantities, and even a solitary observation like that of Mr. Winter during the eclipse of 1871 will give some results."

In an elegant mathematical investigation, complete in all details, Professor Schuster then proceeds to demonstrate these propositions. But the measurements requisite to utilise his results did not immediately follow, probably because the difficulties of obtaining them by visual methods were too great. The gradual introduction of photographic methods, and especially the invention of the "dry-plate," has made them much easier, and at recent eclipses photographs have been obtained which will probably, when suitably measured, give the required information. As yet, however, measures of the polarisation (marked (*a*) above have not been carried out, and the complete problem formulated by Professor Schuster has not yet been solved. But extensive measures of class (*b*), the total brightness of the corona at different distances from the sun, were made on photographs taken in 1893* and 1898† (others have been made but are not yet published), and an approximate law was deduced as follows:

The brightness of the Corona is inversely proportional to the sixth power of the distance from the sun's centre.

Now, making use of Professor Schuster's paper above referred to, we can immediately deduce from this that the particles must be distributed according to the inverse $4\frac{1}{2}$ power of the distance (see Note I.), and I propose now

* *Phil. Trans.*, vol. 66, p. 403.
† *Phil. Trans.*, vol. 68, p. 36.

to examine a little more in detail what further information this gives us about them.

But to prevent misconception, I must recur to one or two points which have been omitted so far in order to simplify the statement. When we come to consider the nature of the Corona, we must remember that it is certainly a complex structure. The spectroscope gives us evidence of the existence of gases which show bright lines in the spectrum. One of these, at least, is a gas unknown to us on earth, which has been called coronium. But besides gases, there are in the Corona solid, or perhaps liquid, particles; for part of the light which comes to us is polarised light, and from the character of the polarisation we can infer the existence of particles of this kind. Moreover we learn something of their approximate size; many of them cannot be very much bigger than a wave length of light, or there would be no such effect. Of course, there may be larger particles than these in the Corona, but these will not polarise the light, and therefore do but dilute the effect. If we find that the polarisation is strong, this is evidence that the number of large particles is small. We see here how important it is to get an actual measure of the amount of polarisation, and measurements of this kind are in progress. We do know, however, that the polarisation is strong, and therefore the small particles numerous, and for the present we are going to consider them alone, as if they constituted the whole Corona. We can afterwards make allowance for the elements neglected.

But the particles are almost certainly not stationary relatively to the sun; they are either rising or falling, or rising *and* falling: and it is into their state of motion that I propose to enquire, in the light of the measurement above quoted.

Let me first take perhaps the simplest case. We have said that the particles must be small. Now Clerk-Maxwell pointed out years ago how the radiation of light would exert a *pressure* on bodies receiving the light, which would be quite insensible for large bodies but might become important for very small ones. This remark has scarcely received proper attention until recently, but in the last year or two Professor Poynting* and others have stated in definite form the amount of this light pressure, and shown that for bodies smaller than two wave lengths of light the pressure may be so great as to counterbalance the sun's gravitational attraction. If so, the particles *may* be continually repelled from the sun, instead of attracted. Repulsion could, of course, also follow from electrical action of the sun. For our purpose these two hypotheses can be considered together. Supposing, then, particles to be *repelled* from the sun, how far would their distribution fit in with the observed law of brightness? To a certain extent, the supposition looks promising; starting from the surface, where the particles may be densely packed, they are spread over a larger and larger surface as they travel outwards. Moreover, since they are travelling with ever-increasing velocity, their density will further diminish owing to this fact. Further, the light received by each diminishes with increasing distance from the sun whether the light be simply scattered sunlight or the incandescent light of the particle itself, heated up as it must be by the intense radiation from the sun: but this is taken account of in Professor Schuster's paper and the deduction we made from it, whereby we converted the law of brightness into a law of density of particles, viz., as the inverse 4½ power

* *Phil. Trans.*, Series A, vol. 202, pp. 525-552; and *Proc. Phil. and Soc. of London*, vol. 19; also *Phil. Mag.*, April, 1905.

of the distance. Combining, then, for comparison with this observed density the two former contributing causes—greater surface, which goes as the square, and greater velocity, which is as the square-root at most*—we get only $2\frac{1}{2}$ instead of $4\frac{1}{2}$: and our supposition does not fit the observed facts. Hence, if particles travelling outwards (under repulsive force) exist in the corona, they can only *dilute* the effect we wish to explain, and we must look for other particles moving in such a way that the index is even greater than $4\frac{1}{2}$, so that it may be reduced to $4\frac{1}{2}$ on the average: just as when, in climbing a hill of which the average slope is known, if we find an easy gradient for some distance, we know that we shall ultimately find an unusually steep part somewhere.

Next let us take another supposition of an inverse kind. Matter cannot be travelling continuously outwards. Can it be, perhaps, travelling continually inwards from space? This supposition is worse than the former; the condensation as we approach the sun being not even so great as before. We still have the "concentration" due to the decrease in surface of concentric spheres, but when we come to the velocity with which a particle crosses any sphere, it is now *greater* near the sun instead of *less*, as before. Accordingly our power of the distance is no longer even so large as $2\frac{1}{2}$, but on the supposition of simple falling, it would be only $1\frac{1}{2}$. There is, however, one new consideration which may be taken into account. Is there anything resisting the falling? We know, for instance, that there is in the Corona a gaseous portion made up of hydrogen, coronium, and perhaps other gases. Do these check the action of the falling particles? When the great eruption of Krakatoa took place twenty years ago, a large amount of dust was flung sky high by the

* See Note I.

volcano, which took years to fall. Many of us remember the beautiful sunset glows of the years near 1884, which were probably due to the existence of this dust high up in our atmosphere, and Professor Stokes showed how the observed rate of fall of these particles was just such as might be inferred from observations of the way in which they scattered light. But if I understand his formula rightly, the rate of fall would be nearly uniform at all heights. It would not at any rate diminish as the particles approach the earth at a rate so marked as the cube of the distance, which is the kind of change we require. We must, of course, be cautious in arguing from our own atmosphere to that of the sun, but in default of positive knowledge to the contrary, I put this supposition aside as not helpful for the illustration of our present problem. [It seems, however, just possible that the resistance of the gaseous atmosphere, combined with a resultant force *outwards*, might satisfy the conditions. This consideration occurred to me too late for full examination.]

Let us, then, turn to some other supposition. Light pressure or electrical repulsion must not overcome gravity, but they may considerably reduce it. One result of this reduction is that a much smaller velocity is necessary to carry a particle to a given height from the sun. If gravity were not so reduced, a velocity of 382 miles per second would be necessary to eject a particle completely from the sun so that it never returns, and a velocity not much less is required to eject it to heights such as are represented in the corona. For instance, to reach a height of one solar diameter the velocity must be 270 miles per second. There is nothing impossible or even unlikely in the existence of such velocities at the sun's surface *a priori*, though presently we shall see some reason for doubting their existence. But for the moment let us

leave on one side the consideration of the absolute magnitude of these velocities, only remarking that a diminution of the sun's attraction is for our purpose equivalent to a diminution in the velocity of projection. We can substitute one for the other and leave the path of the particle practically unaltered; so that we lose no generality by asking how particles ejected with different velocities and in different directions would be distributed, keeping their size and the sun's attraction the same. For simplicity let us restrict the problem further still and consider the variation in magnitude and direction separately. Let us first suppose a velocity given in magnitude but varying in direction. Take, for instance, a velocity just sufficient to project a particle vertically to the height of one radius of the sun; then the same velocity, with horizontal projection, would send the particle skimming round the sun completely as a very close satellite; while intermediate directions would cause it to describe trajectories of different heights; all, however, less than one radius. If particles be shot out from a point on the surface in all directions with this velocity, there will certainly be more of them at any moment near the surface than far away. Beyond one radius there are none at all, and just within that limit there are only the few projected nearly vertically. Lower down we get a greater number from directions more and more inclined to the vertical, and so as we approach the sun the density increases. Close to the sun, indeed, it becomes infinite; for the particles which come skimming along the surface occupy that region for a finite time, whereas all other particles are at any particular distance for an infinitesimal time only. Working out the law of density mathematically, we find that it is not unlike that required, excluding the little shell close to the surface which may be regarded as part of the chromo-

sphere (see Note III.) But the early promise of this supposition is not followed up. Firstly, it would only give us a corona one radius deep and the observed corona is much bigger than this; accordingly we must increase our possible velocity so that particles may rise several radii from the sun, and on coming to this more general case we find quite a different law of distribution. Let us take, for example, a velocity which would carry a particle to the height of three radii vertically. If particles were spouted from a point on the sun's surface in all directions with this velocity, the density at the surface of the sun comes out infinite as before; at a little distance it is finite but decreasing very slowly. The decrease ultimately stops, and then becomes an increase; and we get *another shell with infinite density*, due to the fact that in the neighbourhood of a point on the other side of the sun from the eruptive centre there is a great accumulation of particles in a part of their orbits where they remain for some time at nearly the same solar level (see Note IV.) Outside this shell the density falls off at the rate we desire, but fails altogether at a height of three radii. This supposition accordingly is totally at variance with the observed facts and we are led to the conclusion that variations in direction of projection apart from variations in amount do not give us an adequate explanation of the corona.

We must fall back on variations in magnitude of velocity. But the work already done has not been thrown away; it has simplified the problem. We have seen that with a given magnitude of velocity there is a certain shell where the density becomes infinite compared with that inside and outside, and hence in adding together the results of velocities differing in magnitude, we need only take account of this particular shell in each case, and we

arrive at a conception of the corona as made up of a series of shells one inside the other, corresponding to different velocities. But this does not accord at all well with its appearance; it would give a stratified corona, whereas the observed corona is distinctly radial in character.

For reasons which would be tedious to give at length, the evidence seems to indicate that variations in *direction* of velocity must be very small, and that we may assume the direction to be approximately vertical. The corona near the sun is formed from *low* vertical jets; that at a distance, from jets which reach a great height; and since the former is so much denser, there must accordingly be many more velocities of small size than of large. The law of degradation of brightness is accordingly now become a law telling us how many more small velocities there are than large ones, and we accordingly return to what was originally said about the magnitude of these velocities.

It may not really be the velocities themselves which vary, but the force of the sun's attraction, as diminished by light pressure. When the light pressure and attraction nearly balance one another, a very slight change in the adjustment will double the difference, and it is obvious that we have here great possibilities for variation in magnitude. When we introduce this variation into our equations (see Note V.), we find a notable effect, somewhat of the kind required, on the distance to which the particles are ejected. If we assume a series of attractive forces, in the ratios 10, 9, 8, 7, 6, 5, then a velocity which would, under force 10, eject a particle to the height of 1 radius from the surface (or 2 from the centre—for comparison with our law it is better to measure from the centre), would, with the forces 9, 8, 7, 6, eject it to 2·2, 2·7, 3·5, 6·0 radii from the centre; while under force 5, the

particle would leave the sun altogether. The scattering of the particles is thus very rapid at distances greater than two radii from the centre. But the same does not hold *within* this distance If we extend the series of forces in the other direction by the numbers 11, 12, 13, 14, 15, the corresponding distances from the centre are 1·83, 1·71, 1·63, 1·56, 1·50: in other words, the inner corona would be too nearly uniform, unless we have some other source of variation. This we must seek in the distribution of the sizes of the particles. We must suppose that those which rise to considerable heights, because light pressure nearly balances the sun's attraction, are comparatively few, while there would be many for which the balance was less complete.

We have, therefore, to a certain extent, only replaced one difficulty by another—or rather, only one enquiry by another. Instead of looking for a cause for a certain distribution of density, we now seek the reason of a certain distribution of size. But I think that we have advanced a step, although we may not have completely solved the problem. That there should be this variation in size near the point where light pressure nearly balances attraction is of the nature of a *vera causa*.

At the risk of appearing to argue in a circle, I now call attention to the effect of this supposition on our estimate of the absolute magnitudes of the velocities. If we are right in regarding the variations of magnitude as taking place in the force and not in the velocity of projection, then the average force is small, and the velocities will also be small. This affords us a loophole of escape from a possible difficulty. If particles were ejected with velocities of this magnitude, we might reasonably expect the corona to change its form, at any rate in detail, within a very short time. In one hour, for

instance, a particle would travel over a million miles—more than a solar diameter: and though a jet may retain the same general appearance, though composed of entirely new matter, we might hope to detect differences of detail in it. Good snapshots of Niagara, for instance, differ in details, however quickly they may succeed each other. Now pictures of the corona at an interval of one hour do not differ in this way, and though we must not lay too much stress on the evidence as yet, it affords a presumption in favour of lower velocities. Hence it is a point gained that light-pressure enables us to look for lower velocities of ejection. For instance, instead of the 382 miles per second necessary to eject a particle completely from the sun, one-tenth of this velocity only is required if the force be 100 times smaller, so that we may now contemplate velocities of ten or twenty miles per second, which would with such force raise a particle to a height of several radii, as taking part in the phenomenon. Now it is interesting to find that we have indications of velocities of this size from a quite independent research, namely, that on the Solar granules. The surface of the sun in a telescope presents a mottled appearance. Forty years ago, Mr. James Nasmyth, in a letter to a member of this society—of which within a few months he became a corresponding member—claimed that he had discovered as an explanation of this mottled appearance, that there were scattered over the sun's surface a number of objects resembling "willow-leaves." Thereupon ensued a curious scientific controversy. Other observers saw the phenomenon indeed which Mr. Nasmyth intended to describe, but found fault with his description. Some preferred the name "rice-grains" for the objects which they admitted were there; others spoke of bits of straw. Their various ideas may be illustrated on the screen. Attention was

after some years distracted from this controversy by the work of the spectroscope, but within the last year our interest has been reawakened in it by some very successful photographs* taken by Mr. Hansky, of St. Petersburg, who has not only photographed these granules, but demonstrated their continuous existence as separate bodies of some kind, moving about among one another. But to identify them as individuals it is necessary to take photographs at intervals of less than one minute, for the movements even in a few seconds are large. By measuring the photographs, Mr. Hansky estimates that the velocities of the granules are something like 10 to 20 miles per second. If we may assume that there are in a vertical direction velocities similar to these horizontal ones, we get just the sort of velocity which would suit the phenomena of the corona on the lines above indicated. I do not lay, however, too much stress on this illustration, if only for the reason that Mr. Hansky's discovery is too recent. But I would add one word further of what may be frankly called speculation in the direction of very low velocities.

If for any reason it were to seem probable that velocities even smaller than these might play a part in the phenomenon of the corona, velocities, let us say, of one mile per second, then we must not forget to take into account the possible effect of the sun's rotation. Supposing, for a moment, that the particles owed their ejection to this rotation alone, we can see that they would be chiefly flung off from the equator and the corona would have the appearance of a comparatively flat extension, which we see at a time of minimum sunspots. The extension in other directions seen when sunspots are numerous might then be due to the introduction of some

**Mitt. der Nicolai-Hauptsternwarte zu Pulkowo*, vol. 1, no. 6.

disturbing cause in addition to the sun's rotation. But for considerations of this kind to have any value, the velocities of ejection must be of the same order as that of a particle rotating at the sun's equator, which is about one mile per second. And for this to be effective in carrying it out to several radii from the sun, we must suppose the attractive force diminished by light pressure to the 100,000th part of itself—a supposition which has not much to recommend it.

To sum up, then, the result of this discussion :—

The observed falling off in light of the Corona must be referred to the fact that light pressure nearly, but not quite, balances gravity for the particles forming the corona ; that the difference accordingly varies in magnitude ; that there are many more particles for which this difference is large than for which it is small ; and that, without any further considerable diversity in conditions, particles ejected vertically from the sun's surface with velocities similar to those observed in the granules might then distribute themselves according to the observed law. It must be remembered that we have not taken account at all of the incandescence of the particles, which may vary according to almost any law of distance. We have started from the fact that the corona shows strong polarisation—in other words, that scattered light must play, at any rate, a considerable part in the appearance—and limited the enquiry to this part of the received light.

The discussion is therefore partial only, and reaches no final conclusion. But it is, perhaps, the more characteristic of eclipse work. We are only slowly spelling out, with long intervals between each letter, the lessons to be learnt from the Corona ; and even those we seem to have mastered we must be ready to modify in the light of new facts.

NOTE I.

In Table II. of Professor Schuster's paper (*Mon. Not.*, vol. 40, p. 55) the brightness of the stronger component at different distances from the sun's centre is calculated for different laws of distribution of particles. To get the whole light $I_0 + I_1$ we must combine Tables II. and III., forming

$$2I_0 - (I_0 - I_1).$$

Let us form this quantity for $\theta = 30°$, *i.e.*, 2 radii from the centre: and $\theta = 90°$ or 1 radius (the sun's limb). The ratio of the distances is thus 2; and the brightnesses observed are in the ratio 2^6 or 64. We have to find what law of density of particles gives us this law of brightness. Writing down the brightnesses for different laws of density from Professor Schuster's paper, we have

VALUE OF $I_0 + I_1$

Law of density	r^0	r^{-2}	r^{-4}	r^{-6}
At distance 1, $I_0 + I_1 =$	3·0	1·54	1·20	1·02
„ „ 2, $I_0 + I_1 =$	1·22	0·131	0·0234	0·0046
Ratio of brightnesses	2·46	11·8	51	222
Ratio as power of 2	1·3	3·6	5·7	7·7
Power of 2 for density	0	2	4	6
Difference	+1·3	+1·6	+1·7	+1·7

Thus if the density falls off as the mth power of the distance, the brightness falls off about as the $(m + 1·7)$th power, when m is near the value we require. And since we have observed

$$m + 1·7 = 6$$

we find

$$m = 4·3.$$

But it is perhaps scarcely advisable to choose as one of the points the actual limb of the sun, since the chromospheric phenomena complicate matters here; and indeed the measurements of brightness were not made quite so close to the sun as this. Let us rather take the pair of points for which $\theta = 52°·5$ and $\theta = 22°·5$.

The distances from the sun's centre are, in terms of the sun's radius, the cosecants of these angles, or 1·26 and 2·61, the ratio of which is 2·07 (log = 0·316). The above table is then modified as follows:—

VALUE OF $I_0 + I_1$.

Law of density	r_0	r^{-2}	r^{-4}	r^{-6}
At distance 1·26, $I_0 + I_1$	= 2·0704	·5969	·2753	·1428
„ „ 2·61, $I_0 + I_1$	= ·9191	·0573	·0058	·0007
Ratio of brightnesses	= 2·25	10·4	47·4	204
Ratio as power of 2·07	= 1·1	3·2	5·3	7·3
Power of 2·07 for density	= 0	2	4	6
Difference.....	1·1	1·2	1·3	1·3

The difference 1·7 has been reduced to 1·3: and the law of density approaches the inverse fifth power instead of the inverse 4th. For our purpose we may take the index as $-4·5$.

NOTE II.

The velocity is given by the equation

$$v^2 = \mu\left(\frac{1}{a} - \frac{2}{r}\right)$$

and thus although v increases with r it is not expressible as a power of r. But we can represent it approximately by a power of r within a limited region. Let R be the Sun's radius. The velocity is zero when $r = 2a$; and if this occurs at the Sun's surface $2a = R$: otherwise $2a < R$, and r is always greater than $2a$. Let us compare the velocities at distances r and $2r$. The ratio $v_2^2 : v_1^2$ is $(r - a) : (r - 2a)$ which tends to unity when r is large compared with a. It is less than 2 unless r is less than $3a$. Thus $v_2 : v_1$ is less than $2^{\frac{1}{2}}$ except within a possible thin shell close to the Sun. This shell does not exist at all if $R > 3a$, that is, if the square of the velocity of projection be greater than μR: and its maximum thickness is $R/2$, when the particles start from rest at the surface.

NOTE III.

The velocity in a trajectory, with sun's centre as focus, is given by the equation

$$v = \mu\left(\frac{2}{r} - \frac{1}{a}\right).$$

At the sun's surface $r = R$, $v = V$ say

$$V^2 = \mu\left(\frac{2}{R} - \frac{1}{a}\right).$$

If this be just sufficient to carry a particle to height R above the surface, when the direction of projection is vertical, then $a = R$ and $\mu = RV^2$.

For any other direction of projection, making an angle θ with the vertical, let the direction of motion make an angle ϕ with the focal distance at any point. The velocity outwards from the sun's centre, *i.e.*, along the focal distance, is $v\cos\phi$. We must express this in terms of r and the initial quantities V and θ. The factor v is given above. As regards ϕ we have, if p be the perpendicular on the tangent from the sun's centre,

$$\sin\phi = \frac{p}{r} = \frac{h}{vr} = \frac{VR\sin\theta}{vr},$$

h being the well-known constant.

Let us now consider the number of particles projected in the direction θ. We may take them as proportional to $2\pi\sin\theta \,.\, d\theta$. Within the spherical shell r to $r + dr$ there will be a number of these proportional to $dr/v\cos\phi$; and

$$\begin{aligned} v^2\cos^2\phi &= v^2 - v^2\sin^2\phi \\ &= RV^2\left(\frac{2}{r} - \frac{1}{R}\right) - \frac{V^2R^2\sin^2\theta}{r^2} \\ &= \frac{V^2}{r^2}(2rR - r^2 - R^2\sin^2\theta) \\ &= \frac{V^2}{r^2}[R^2\cos^2\theta - (r - R)^2] \end{aligned}$$

Hence for the density within the shell, whose volume is $4\pi r^2 dr$, we shall have

$$\frac{K}{Vr} \cdot \frac{\sin\theta d\theta}{[R^2\cos^2\theta - (r - R)^2]^{\frac{1}{2}}}$$

where K is a constant depending on the number of particles ejected in unit time. To get the total density we must integrate this expression between limits. The lower limit is clearly $\theta = 0$. For the upper, we must include all directions of projection providing particles which reach the shell; some do not. The direction for which a particle just reaches the shell is given by

$$\cos\theta = \frac{r-R}{R} = \cos\alpha, \text{ say}$$

and for larger values of θ than this the denominator of the expression to be integrated becomes imaginary.

For the integration, put

$$R\cos\theta = (r-R)\sec\psi$$
$$-R\sin\theta d\theta = (r-R)\sin\psi\sec^2\psi d\psi$$
$$[R^2\cos^2\theta - (r-R^2]^{\frac{1}{2}} = (r-R)\tan\psi$$

and we get, since $R\cos\alpha = (r-R)$,

$$N = \frac{K}{VRr}\int_0^{\alpha}\sec\psi d\psi$$
$$= \frac{K}{VRr}\log_e\tan\left(\frac{\pi}{4}+\frac{\alpha}{2}\right)$$
$$= \frac{K'}{VRr}\log_{10}\tan\left(\frac{\pi}{4}+\frac{\alpha}{2}\right)$$

where $R\cos\alpha = r-R$, and K' is a new constant.

This becomes infinite at the sun's surface, *i.e.*, when $r = R$: which is otherwise obvious since a number of particles remain at this distance for an indefinite time as close satellites. For distances greater than this we can quickly calculate

$$\log_{10}\tan\left(\frac{\pi}{4}+\frac{\alpha}{2}\right) = L$$

say, as in Table I. If L varies at any point as r^{-n}, then

$$\log L + n\log r = \text{const.},$$

and hence differences of $\log L$, divided by differences of $\log r$, will give n, as in the last column of Table I. The average value of n is about 3 up to distance 1·8, where it rapidly increases.

Hence the density of particles, which varies as L/r, or as $1/r^{n+1}$ falls off about as the inverse *fourth* power of the distance up to $r = 1{\cdot}8$, and then much more rapidly.

TABLE I.

r/R.	α.	$\log_{10}\tan\left(\frac{\pi}{4}+\frac{\alpha}{2}\right)$	$\log L$.	Differences.	$\log r/R$.	Differences.	Ratio.
1·1	84°·2	1·2954	0·1123		·0414		
				·1153		·0378	3·1
1·2	78·5	0·9932	9·9970		·0792		
				·0857		·0347	2·5
1·3	72·6	0·8152	9·9113		·1139		
				·0788		·0322	2·5
1·4	66·4	0·6800	9·8325		·1461		
				·0752		·0300	2·5
1·5	60·0	0·5719	9·7573		·1761		
				·0779		·0280	2·8
1·6	53·1	0·4780	9·6794		·2041		
				·0893		·0263	3·4
1·7	45·6	0·3892	9·5901		·2304		
				·1124		·0249	4·5
1·8	36·9	0·3004	9·4777		·2553		
				·1712		·0235	7·3
1·9	25·8	0·2025	9·3065		·2788		
				∝		·0222	∝
2·0	0·0	0·0000	∝		·3010		

NOTE IV.

If we do not take the special case where $a = R$, we arrive at the integral

$$\frac{K}{Vr}\int \frac{\sin\theta\, d\theta}{\{(2aR - R^2)\cos^2\theta + Z\}^{\frac{1}{2}}}$$

where

$$Z = (a-R)^2 - (r-a)^2$$

and is positive if

$$a - R > r - a \quad \text{or} \quad r < 2a - R;$$

negative if

$$r > 2a - R.$$

We thus have two cases to deal with in the integration.

Case I. $r > 2a - R$, $Z = -\rho^2$ say.

This case bears the closer resemblance to that of Note II. The limit for ϕ is determined by the vanishing of the denominator. Putting $2aR - R^2 = p^2$, the denominator vanishes when

$$p\cos\theta = q.$$

Let α denote this value of θ. Then the integral becomes

$$\frac{K}{b r_1} \int_0^{\alpha} \frac{\sin\theta d\theta}{\sqrt{p^2\cos^2\theta - q^2 + \rho^2}}$$

which is the same as that considered in Note II., with ρ written for R and γ for $r - R$. The value is thus

$$\frac{K}{\rho b r} \log_e \tan\left(\frac{\pi}{4} + \frac{\alpha}{2}\right).$$

Since $\rho^2 = (r - a)^2 - (a - R)^2$ and vanishes when $r = 2a - R$, the density becomes infinite at this distance, as it did in Note II. for $r = R$. Outside this distance the density falls off in a manner somewhat similar to that already tabulated for Note II. It is scarcely necessary to give tables, which would have to be made for different values of a, since the study of Case II. shews us that the original supposition will not fit the facts. But if tables are required, perhaps the quickest way of getting one for any value of a would be to utilise Table I., keeping the values of α the same and calculating the corresponding values of r from the equation

$$p\cos\alpha = q$$

or

$$R(2a - R)\cos^2\alpha = (r - R)(r - \overline{2a - R})$$

or

$$(r - a)^2 = a^2 - R(2a - R)\sin^2\alpha.$$

For instance if $a = 2R$, we have

$$r, R = 2 + (4 - 3\sin^2\alpha)^{\frac{1}{2}}$$
$$= 2 + (1 + 3\cos^2\alpha)^{\frac{1}{2}}.$$

Hence we should get a table as follows, knowing that the first column of Table I. is $1 + \cos\alpha$.

The ratio in the last column, increased by unity, indicates the inverse power of the distance at which the density falls off, and we see that the power is much higher than before.

TABLE II.

Cos α.	$(1+3\cos^2\alpha)$.	$r/R =$ sq. root + 2.	log. r/R.	Differences.	Difference of L.	Ratio.
0·1	1·03	3·02	·480			
				·006	·1153	19
0·2	1·12	3·06	·486			
				·010	·0857	8·6
0·3	1·27	3·13	·496			
				·012	·0788	6·6
0·4	1·48	3·22	·508			
				·013	·0752	5·8
0·5	1·75	3·32	·521			
				·016	·0779	4·9
0·6	2·08	3·44	·537			
				·016	·0893	5·6
0·7	2·47	3·57	·553			
				·016	·1124	7·0
0·8	2·92	3·71	·569			
				·017	·1712	10·0
0·9	3·43	3·85	·586			
				·016	∞	α
1·0	4·00	4·00	·602			

We now come to consider

Case II. When $2a - R > r > R$ the integral becomes

$$N = \frac{K}{\sqrt{r}} \int \frac{\sin\theta d\theta}{(p^2\cos^2\theta + s^2)^{\frac{1}{4}}}$$

where

$$s^2 = (a - R)^2 - (r - a)^2.$$

The limits are now

$$\theta = 0 \text{ to } \theta = \frac{\pi}{2},$$

since all directions of projection are represented in the shell of radius r. Put

$$p\cos\theta = s\tan\psi$$
$$-p\sin\theta d\theta = s\sec^2\psi d\psi$$
$$(p^2\cos^2\theta + s^2)^{\frac{1}{2}} = s\sec\psi.$$

For limits, when $\theta = 0$, $s\tan\psi = p$. Let this value of ψ be denoted by λ.

When

$$\theta = \frac{\pi}{2}, \quad \psi = 0.$$

Hence

$$N = \frac{K}{\sqrt{rp}}\int_0^{\lambda}\sec\psi d\psi$$

$$= \frac{K}{\sqrt{rp}}\log_e\tan\left(\frac{\pi}{4} + \frac{\lambda}{2}\right)$$

a similar expression to those obtained previously and it is most easily tabulated by taking $\cot\lambda$ as the argument. We have to find r from the equation

$$s^2\tan^2\lambda = p^2$$

or

$$(r-a)^2 = (a-R)^2 - (2aR - R^2)\cot^2\lambda.$$

Let us take the same particular value of a as in Case I., viz., $a = 2R$. Then

$$r/R = 2 \pm (1 - 3\cot^2\lambda)^{\frac{1}{2}}.$$

Hence $\tan^2\lambda$ is greater than 3, or λ lies between 60° and 90°. The former table only contains 5 values of λ (or a) between these limits, but they will suffice to illustrate the solution to be

expected, since we may give either sign to the radical, or what is the same thing, the solution for $(a-r)$ is the same as that for $(r-a)$.

TABLE III.

$\cot\lambda$.	$1-3\cot^2\lambda$.	r/R.	$\log r/R$.	Differences.	Diff. $\log L$.	Ratio.
·102	·969	2·985	·4749			
				·0073	−·1153	−15·8
·204	·875	2·935	·4676			
				·0143	−·0857	−6·0
·313	·706	2·840	·4533			
				·0294	−·0788	−2·7
·437	·427	2·654	·4239			
				·1228	−·0752	−0·6
·577	·000	2·000	·3010			
				·1720	+·0752	+0·4
·437	·427	1·346	·1290			
				·0645	+·0788	+1·2
·313	·706	1·160	·0645			
				·0371	+·0857	+2·3
·204	·875	1·065	·0274			
				·0210	+·1153	+5·5
·102	·969	1·015	·0064			

Hence the density falls at first, being infinite at the surface; but when $r=2R$ it ceases to decrease and begins to increase, becoming infinite again when $r=2a-R$ or $r=3R$.

NOTE V.

The variation in height attained for different velocities follows at once from the equation

$$v^2=\mu\left(\frac{2}{r}-\frac{1}{a}\right)$$

For vertical projection, the maximum height, given by $v=0$, is $r=2a$. Suppose this is one radius (R) from the surface, or

$2R$ from the centre, so that $a = R$: and that in this case $\mu = 10$. Then the velocity V at the surface ($r = R$) is given by

$$V^2 = 10\left(\frac{2}{R} - \frac{1}{R}\right) = 10/R.$$

For any other force μ and the same initial velocity $10/R$ we have

$$\frac{R}{a} = 2 - \frac{10}{\mu}.$$

Putting $\mu = 9$, 8, &c., in succession, we get the results in the text.

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VIII. On the Range of Progressive Waves of Finite Amplitude in Deep Water.

By R. F. Gwyther, M.A.

(Communicated by Professor H. Lamb, LL.D., D.Sc., F.R.S.)

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The waves considered in this paper are those first, and very fully, discussed by Stokes,* in his paper on the "Theory of Oscillatory Waves," and in the "Supplement" to that paper.

The object of the investigation is to confirm the hypothesis that this class of wave is mathematically capable of propagation with uniform velocity and without change of form, and to determine the limit of the range of such waves in height, and as far as possible, the change in shape of the profile of the wave as the ratio of the height to the wave length is increased.

The method which I employ is that of tracing the paths of the fluid particles. It is not supposed that the knowledge of these paths is of interest; but there seems no more complete method of testing whether the motion assumed can actually take place than one which shews how the motions of the particles are to be coordinated in order to adapt themselves to the circumstances of the case. Since this investigation shews that the motions of the individual particles can, within a certain range, be traced to any desirable degree of accuracy, it follows that this is a form of wave of finite amplitude which represents an actually possible phenomenon.

* *Mathematical and Physical Papers*, vol. I., p. 196, and *Supplement*, p. 314.

May 31st, 1906.

THE EXPANSION OF THE COEFFICIENTS IN THE SERIES.

Although the formal expressions for x, y, and u (or ϕ) have now been obtained in terms of μ and therefore of t, it is still necessary to express these coefficients in ascending powers of H_1 and h_1, before the series add anything to our knowledge of the motion of the fluid in Stokes' problem.

It is unlikely that this step can be performed by any method without a considerable amount of labour. In order to retain, in all cases, and as conveniently as may be, expansions which proceed in sines and cosines of multiples of ϕ, I adopt the method of expanding

$$\cos r(E_1 \sin\phi + E_2 \sin 2\phi + \ldots),$$

and

$$\sin r(E_1 \sin\phi + E_2 \sin 2\phi + \ldots),$$

by means of series in which the coefficients are Bessel's Functions.

The relations which I continually employ are

$$\cos(\lambda \sin\psi) = J_0(\lambda) + 2\Sigma J_{2m}(\lambda)\cos 2m\psi$$

and

$$\sin(\lambda \sin\psi) = 2\Sigma J_{2m-1}(\lambda)\sin(2m-1)\psi.$$

I shall not give the details of the work, but write down the full expansions to the 6th degree of approximation. The only simplification which I make is to replace $J_0(\lambda)$ by unity in cases where λ^2 occurs to a sufficiently high power to justify my doing so.

The expansions are

$$\cos r\{E_1 \sin\phi + E_2 \sin 2\phi + \ldots + E_6 \sin 6\phi\}$$
$$= J_0(rE_1).J_0(rE_2).J_0(rE_6) - 2.J_2(rE_1)J_1(rE_3).$$

case tends. An attempt is made to shew (and I hope the argument is sound) that all progressive waves in deep water are of the class which possesses a horizontal series of poles above the surface of the water, and that there is no limit to the closeness to the water surface which this series of poles may assume. As this process is continued the point of inflexion of the wave surface continually approaches the crest, and in the final stage coincides with it, and the wave profile shews a finite angle at the crest.

In this critical case, the wave-profile is the same as that investigated by Mitchell * in his paper on "The Highest Wave in Water."

The Solution of Lagrange's Equations.

The functional solution of Lagrange's equations for irrotational motion of a fluid in two dimensions used in this paper is found as follows.

Since the equations of condition may be written in the form

$$\begin{vmatrix} \frac{\partial}{\partial a}(\dot{x}+i\dot{y}) & \frac{\partial}{\partial b}(\dot{x}+i\dot{y}) \\ \frac{\partial}{\partial a}(x-iy) & \frac{\partial}{\partial b}(x-iy) \end{vmatrix} = 0,$$

$\dot{x}+i\dot{y}$ must be a function of $x-iy$ and t, and in the case of steady motion which I am here considering, a function of $x-iy$ only.

Writing

$$\frac{d}{dt}(x+iy) = \phi'(x-iy),$$

we get

$$\phi'(x+iy)\frac{d}{dt}(x+iy) = \phi'(x+iy)\phi'(x-iy)$$

$$= \text{a real quantity,}$$

* *Phil. Mag.*, November, 1893.

and therefore

$$\phi(x+iy)=u+ib,$$

where u is real and b is independent of t.

If we suppose now that this relation can be reversed, we may write

$$x+iy=f(u+ib) \quad . \quad . \quad . \quad (1).$$

This is, of course, exactly comparable with the Eulerian relation usually written

$$x+iy=f(\phi+i\psi).$$

Proceeding again with this relation, we have

$$\frac{d}{dt}(x+iy)=f'(u+ib)\frac{du}{dt}.$$

This must be a function of $x-iy$, and therefore of $u-ib$, and it can only take the form $c/f'(u-ib)$, where c is an absolute constant.

We therefore must have

$$f'(u+ib)f'(u-ib)du=cdt$$

and

$$\int f'(u+ib)f'(u-ib)du=ct+a \quad . \quad . \quad (2).$$

This relation gives, in any case, u as a function of a and b, and these symbols in the Lagrangian equations are now introduced as constants of integration.

I now proceed to show that continuing to use the Lagrangian method, the expression for the pressure takes the same form as that derived from the Eulerian method. This is solely a matter of form, since the result could not be otherwise.

The equations with which we have to deal are

$$\frac{\partial}{\partial a}\left\{\frac{p}{\rho}-gy-\tfrac{1}{2}(\dot{x}^2+\dot{y}^2)\right\}+\frac{d}{dt}\left\{\dot{x}\frac{\partial x}{\partial a}+\dot{y}\frac{\partial y}{\partial a}\right\}=0. \qquad \text{etc.}$$

We have

$$\dot{x}^2+\dot{y}^2=c\dot{u},$$

$$\dot{x}\frac{\partial x}{\partial a}+\dot{y}\frac{\partial y}{\partial a}=c\frac{\partial u}{\partial a},$$

$$\dot{x}\frac{\delta x}{\delta b}+\dot{y}\frac{\delta y}{\delta b}=c\frac{\delta u}{\delta b},\text{ so that}$$

$$\frac{\delta}{\delta a}\left\{\frac{p}{\rho}-gy-\tfrac{1}{2}c\dot{u}\right\}+\frac{d}{dt}\left(c\frac{\delta u}{\delta a}\right)=0$$

with a precisely similar equation.

The condition that the motion shall be irrotational requires that c shall be independent of a and b, which is here satisfied since c is an absolute constant.

The pressure is given by

$$\frac{p}{\rho}-gy+\tfrac{1}{2}c\dot{u}=\text{constant}$$

or

$$\frac{p}{\rho}-gy+\frac{c^2}{2f'(u+ib)f'(u-ib)}=\text{constant} \quad . \quad . \quad (3).$$

It follows from this that if $x+iy=f(\phi+i\psi)$ is a solution of the Eulerian equations which can be made to satisfy the conditions of a steady motion, then

$$x+iy=f(u+ib)$$

where

$$\int f'(u+ib)f'(u-ib)du=ct+a,$$

is the solution of the Lagrangian equations under the same circumstances.

Application to Stokes' Problem.

The mode of treatment of waves of finite amplitude in deep water employed by Sir George Stokes in his "Supplement to a paper on the Theory of Oscillatory Waves"* leaves the question in the form to which the method of the last chapter is applicable.

Confining myself to the steady motion, from which the progressive wave is to be obtained in the usual way, I write, following Stokes,

$$x+iy=f(u+ib)=u+ib-i\Sigma\frac{h_n}{nk}e^{nk(u+ib)} \quad . \quad . \quad (4),$$

* *Mathematical and Physical Papers*, vol. i., p. 314.

where n is an integer, and k is a quantity taken as defining the wave length and is a constant.

From this it follows that

$$f'(u+ib) = 1 + \Sigma h_n e^{-nkb} e^{inku} = 1 + \Sigma H_n e^{inku} \quad . \quad (5),$$

where

$$H_n = h_n e^{-nkb} \quad . \quad . \quad . \quad . \quad (6).$$

Since the surface is, in the steady motion, the path of a fluid particle, it is one of the lines for which b = constant, and we may most conveniently select this to be the line from which b originates, so that $b=0$ defines the free surface of the fluid.

The condition necessary to secure the constancy of pressure at the free surface is, from (3), that

$$2gy - c^2/f'(u+ib)f'(u-ib)$$

shall be independent of u, when $b=0$.

This requires that

$$2g\Sigma\frac{1}{n}h_n \cos nku + kc^2(1+\Sigma h_n e^{inku})^{-1}(1+\Sigma h_n e^{-inku})^{-1}$$

shall be independent of u.

The determination of the resulting relations to any desired degree of approximation, after the manner of Stokes, is merely a matter of labour. I have carried the approximation to the 6th order, in order to show rather more of the character of the final solution of the Lagrangian equations. Stating the results only, I obtain

$$\begin{aligned}
kc^2/g &= 1 + h_1^2 + \tfrac{7}{2}h_1^4 + \tfrac{229}{12}h_1^6, \\
h_2 &= 2h_1^2 + h_1^4 + \tfrac{29}{6}h_1^6, \\
h_3 &= \tfrac{3}{2}h_1^3 + \tfrac{19}{4}h_1^5, \\
h_4 &= \tfrac{13}{3}h_1^4 + \tfrac{131}{15}h_1^6, \\
h_5 &= \tfrac{625}{24}h_1^5, \\
h_6 &= \tfrac{1369}{30}h_1^6 \quad . \quad . \quad . \quad . \quad . \quad (7)
\end{aligned}$$

On comparing with Stokes' results (*loc. cit.*, p. 318), and allowing for the change in the form of the coefficients which I employ, the results will be found to agree as far the 4th order.

If $2a$ stands for the height of the wave from trough to crest, we get from (7) and (4)

$$ka = h + \tfrac{3}{2}h^3 + \tfrac{1}{2}\tfrac{6}{4}\tfrac{5}{4}h^5.$$

The next stage in the process is to apply the condition (2) to express the necessary relation between u, a, b, and t.

This requires

$$\int (1 + \Sigma H_n e^{in\phi})(1 + \Sigma H_n e^{-in\phi})du = ct + a$$

or

$$\int [\{1 + \Sigma H_n^2\} + 2\{H_1 + \Sigma H_n H_{n+1}\}\cos ku + 2\{H_2 + \Sigma H_n H_{n+2}\}\cos 2ku + \ldots]du = ct + a.$$

This becomes

$$\{1 + \Sigma H_n^2\}ku + \tfrac{2}{1}\{H_1 + \Sigma H_n H_{n+1}\}\sin ku + \tfrac{2}{2}\{H^2 + \Sigma H_n H_{n+2}\}\sin 2ku + \ldots = k(ct + a).$$

For convenience, I shall divide by the coefficient of ku, and write this

$$\phi + E_1 \sin\phi + E_2 \sin 2\phi + \ldots = \mu \quad . \quad . \quad (8),$$

where

$$\phi = ku,$$

$$\mu = k(ct + a)/(1 + \Sigma H_n^2),$$

$$E_1 = 2\{H_1 + \Sigma H_n H_{n+1}\}/(1 \cdot \{1 + \Sigma H_n^2\}),$$

$$E_2 = 2\{H_2 + \Sigma H_n H_{n+2}\}/(2 \cdot \{1 + \Sigma H_n^2\}), \quad \text{etc.}$$

These expressions contain, though not in a very convenient form, the connections which we are seeking, and the solution is contained in (8) and the relations

$$kx = \phi + \Sigma \frac{H_n}{n} \sin n\phi,$$

$$ky = kb - \Sigma \frac{H_n}{n} \cos n\phi.$$

———

THE REVERSAL OF THE SERIES.

The next stage in the process is to shew how to express ϕ, and finally x and y, explicitly in terms of μ, and therefore in terms of t. The whole of the results thus obtained as far as (20), are generally applicable in cases of wave motion, and are independent of the special values of the constants which are introduced at the next stage.

It will be noted that the relation (8) is of the same character, though of course more complex, as that connecting the mean and the eccentric anomaly, and that it is capable of reversal by the method which Bessel applied to the astronomical problem. The same method may also be applied to express x and y in terms of t.

The relation (8) is

$$\phi + E_1 \sin\phi + E_2 \sin 2\phi + \ldots \quad = \mu,$$

hence when $\phi = r\pi$ we have $\mu = r\pi$, and we may properly assume

$$\phi = \mu + A_1 \sin\mu + A_2 \sin 2\mu + \ldots \qquad (9).$$

Differentiating this last equation with respect to μ, multiplying throughout by $\cos r\mu$ (where r is any positive integer), and then integrating with respect to μ between the limits 0 and π of μ, we remain with only the term arising from $\sin r\mu$ in the series in (7).

Accordingly we obtain

$$\frac{\pi}{2} r A_r = \int_0^\pi \left(\frac{d\phi}{d\mu} - 1\right) \cos r\mu \, d\mu$$

$$= \int_0^\pi \frac{d\phi}{d\mu} \cos r\mu \, d\mu,$$

and since, as we have noted, ϕ has the value $r\pi$ when μ has the value $r\pi$, it follows that we may change the independent variable from μ to ϕ, and retain the limits unchanged.

Hence

$$\frac{\pi}{2} r A_r = \int_0^{\pi} \cos r\mu d\phi$$

$$= \int_0^{\pi} \cos r(\phi + E_1 \sin\phi + E_2 \sin 2\phi + \ldots) d\phi \quad . \quad (10).$$

The series for $kx - \phi$ can also be similarly replaced by a series in terms of sines of multiples of μ.

As before we are entitled to assume

$$kx - \phi = M_1 \sin\mu + M_2 \sin 2\mu + \ldots \qquad (11).$$

Treating this equation as we have just treated (9), we obtain

$$\frac{\pi}{2} r M_r = \int_0^{\pi} \frac{d}{d\mu}(kx - \phi) \cos r\mu d\mu$$

$$= \int_0^{\pi} \frac{d}{d\phi}(kx - \phi) \cos r\mu d\phi$$

$$= \int_0^{\pi} \Sigma H_m \cos m\phi \cos r(\phi + E_1 \sin\phi + E_2 \sin 2\phi \ldots) d\phi$$
. (12).

Finally the form properly to be assumed for $k(y - b)$ is

$$k(y - b) = -N_1 \cos\mu - N_2 \cos 2\mu - \ldots \qquad (13).$$

and, as before,

$$\frac{\pi}{2} r N_r = \int_0^{\pi} \frac{d}{d\mu}(ky) \sin r\mu d\mu$$

$$= \int_0^{\pi} \frac{d}{a\phi}(ky) \sin r\mu d\phi$$

$$= \int_0^{\pi} \Sigma H_m \sin m\phi \sin r(\phi + E_1 \sin\phi + E_2 \sin 2\phi + \ldots) d\phi \ldots$$
. (14).

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The expansions are

$$\cos r\{E_1 \sin\phi + E_2 \sin 2\phi + \ldots + E_6 \sin 6\phi\}$$
$$= J_0(rE_1).J_0(rE_2)J_0(rE_6) - 2J_2(rE_1)J_1(rE_3).$$

$$
\begin{aligned}
&- 2J_1(rE_2)\{J_1(rE_1) + J_3(rE_1) + J_1(rE_3)\}\cos\phi \\
&+ \{2J_2(rE_1)[J_0(rE_2) + J_2(rE_2)] - J_1(rE_1)J_1(rE_3) \\
&\qquad - J_1(rE_2)J_1(rE_4)\}\cos 2\phi \\
&+ 2J_1(rE_1)\{J_1(rE_2) - J_1(rE_4)\}\cos 3\phi \\
&+ 2\{J_4(rE_1) + J_0(rE_1)J_2(rE_2) + J_1(rE_1)[J_1(rE_3) \\
&\qquad - J_1(rE_5)]\}\cos 4\phi \\
&+ 2\{J_1(rE_1)J_1(rE_4) + J_1(rE_2)[J_2(rE_1) + J_1(rE_2)]\}\cos 5\phi \\
&+ 2\{J_6(rE_1) + J_2(rE_2) + J_2(rE_1)J_2(rE_2) + J_2(rE_1)J_1(rE_3) \\
&\qquad + J_1(rE_1)J_1(rE_5) + J_1(rE_2)J_1(rE_4)\}\cos 6\phi \quad . \quad . \quad (15),
\end{aligned}
$$

and

$$
\begin{aligned}
&\sin r\{E_1\sin\phi + E_2\sin 2\phi + \ldots \quad + E_6\sin 6\phi\} \\
&\quad = 2\{J_1(rE_1)J_0(rE_2) + J_2(rE_1)J_1(rE_2)\}\sin\phi \\
&+ 2\{J_1(rE_2)[J_0(rE_1) - J_4(rE_1) - J_1(rE_1)J_1(rE_3)] \\
&\qquad + J_2(rE_1)J_1(rE_4)\}\sin 2\phi \\
&+ 2\{J_3(rE_1) - J_1(rE_1)J_2(rE_2) + J_0(rE_1)J_1(rE_3)\}\sin 3\phi \\
&+ 2\{J_1(rE_2)[J_2(rE_1) - J_1(rE_1)J_1(rE_3)] + J_0(rE_1)J_1(rE_4)\}\sin 4\phi \\
&+ 2\{J_5(rE_1) + J_1(rE_3) + J_1(rE_1)J_2(rE_3) \\
&\qquad + J_2(rE_1)J_1(rE_2)\}\sin 5\phi \\
&+ 2\{J_1(rE_6) + J_3(rE_2) + J_1(rE_2)[J_4(rE_1) + J_1(rE_1)J_1(rE_3)] \\
&\qquad + J_2(rE_1)J_1(rE_4)\}\sin 6\phi \quad . \quad . \quad (16)
\end{aligned}
$$

The coefficients A_1, A_2...of the expansion of ϕ in (8) can now be written down. They are

$$
\begin{aligned}
A_1 = &- 2J_1(E_2)\{J_1(E_1) + J_3(E_1) + J_1(E_3)\} \\
&- 2\{J_1(E_1)J_0(E_2) + J_2(E_1)J_1(E_2)\}, \\
2A_2 = &\quad 2\{J_2(2E_1)[J_0(2E_2) + J_2(2E_2)] - J_1(2E_1)J_1(2E_3) \\
&\qquad - J_1(2E_2)J_1(2E_4)\} \\
&- 2\{J_1(2E_2)[J_0(2E_1) - J_4(2E_1) - J_1(2E_1)J_1(2E_3)] \\
&\qquad + J_2(2E_1)J_1(2E_4)\} \\
3A_3 = &\quad 2J_1(3E_1)\{J_1(3E_2) - J_1(3E_4)\} \\
&- 2\{J_3(3E_1) + J_0(3E_1)J_1(3E_3) - J_1(3E_1)J_2(3E_3)\}
\end{aligned}
$$

$$4A_4 = 2\{J_4(4E_1) + J_0(4E_1)J_2(4E_2) + J_1(4E_1)[J_1(4E_2) - J_3(4E_2)]\}$$
$$- 2\{J_0(4E_1)J_1(4E_4) + J_1(4E_2)[J_2(4E_1) - J_1(4E_1)J_1(4E_2)]\}$$

$$5A_5 = 2\{J_1(5E_1)J_1(5E_4) + J_1(5E_2)[J_2(5E_1) + J_1(5E_3)]\}$$
$$- 2\{J_3(5E_1) + J_1(5E_5) + J_1(5E_1)J_2(5E_2) + J_2(5E_1)J_1(5E_2)\}$$

$$6A_6 = 2\{J_6(6E_1) + J_2(6E_2) + J_2(6E_1)J_2(6E_2) + J_2(6E_1)J_1(6E_3) + J_1(6E_1)J_1(6E_5) + J_1(6E_2)J_1(6E_4)\}$$
$$- 2\{J_1(6E_6) + J_3(6E_2) + J_1(6E_2)[J_4(6E_1) + J_1(6E_1)J_1(6E_2)] + J_2(6E_1)J_1(6E_4)\} \ldots \quad (17)$$

The coefficients M_r and N_r in (11) and (13) can best be found in conjunction, thus

$$\frac{\pi r}{2}\{M_r + N_r\} = \int_0^\pi \Sigma H_m \cos\{(r-m)\phi + r(E_1 \sin\phi + \ldots)\}d\phi \ldots (18)$$

and

$$\frac{\pi r}{2}\{M_r - N_r\} = \int_0^\pi \Sigma H_m \cos\{(r+m)\phi + r(E_1 \sin\phi + \ldots)\}d\phi \ldots (19)$$

and the values of each integral can readily be written down by means of (15) and (16).

I thus obtain

$$M_1 + N_1 = 2H_1J_0(E_1)J_0(E_2) + 2H_2J_1(E_1)[1 - J_1(E_2)] + 2H_3\{J_2(E_1) + J_1(E_2)\},$$

$$M_1 - N_1 = 2H_1\{J_2(E_1) - J_1(E_1)J_1(E_3) - J_0(E_1)J_1(E_2)\} + 2H_2\{J_1(E_1)J_1(E_2) - J_3(E_1) - J_1(E_3)\},$$

$$2(M_2 + N_2) = -2H_1\{J_1(2E_2)[J_1(2E_1) + J_3(2E_1) + J_1(2E_3)] + J_1(2E_1)J_0(2E_2) + J_2(2E_1)J_1(2E_3)\} + 2H_2J_0(2E_1)J_0(2E_2) + 2H_3J_1(2E_1)\{1 - J_1(2E_2)\} + 2H_4\{J_2(2E_1) + J_1(2E_2)\}$$

$$2(M_2 - N_2) = 2H_1\{J_1(2E_1)[J(2E_2) + J_2(2F_2) - J_1(2E_4)] - J_3(2E_1) - J_0(2E_1)J_1(2E_3)\} + 2H_2\{J_4(2E_1) + J_3(2E_2) - J_1(2F_4) + J_1(2E_1)J_1(2F_3) - J_1(2E_2)J_2(2E_1)\},$$

$$3(M_3 + N_3) = 2H_1\{J_2(3E_1) - J_0(3E_1)J_1(3E_2) - J_1(3E_1)J_1(3E_3)\} - 2H_2J_1(3E_1)\{1 + J_1(3E_1)\} + 2H_3J_0(3E_1) + 2H_4J_1(3E_1),$$

$$3(M_3 - N_3) = 2H_1\{J_4(3E_1) + J_2(3E_2) - J_1(3E_4) + J_1(3E_1)J_1(3F_3) - J_1(3E_2)J_2(3E_1)\},$$

$$4(M_4 + N_4) = 2H_1\{J_1(4E_1)[J_1(4E_2) + J_2(4F_2) - J_1(4E_4)] - J_3(4E_1) - J_0(4E_1)J_1(4E_3)\} + 2H_2\{J_3(4E_1) - J_1(4E_1)J_1(4K_3) - J_1(4E_2)J_0(4E_1)\} - 2H_3J_1(4E_1)\{1 + J_1(4E_2)\} + 2H_4J_0(4E_1) + 2H_5J_1(4E_1)$$

$$4(M_4 - N_4) = 2H_1\{J_1(4F_1)[J_1(4E_4) - J_2(4E_2)] + J_1(4E_2)[J_3(4E_1) + J_1(4E_3)] - J_3(4E_1) - J_1(4E_3) - J_2(4E_1)J_1(4E_2)\}$$

$$5(M_5 + N_5) = 2H_1\{J_4(5E_1) - J_1(5E_4) + J_2(5E_2) + J_1(5E_1)J_1(5E_3) - J_1(5E_3)J_2(5E_1)\} + 2H_2\{J_1(5E_1)J_1(5E_2) - J(5E_1) - J_1(5E_3)\} + 2H_3\{J_2(5E_1) - J_1(5E_2)\} - 2H_4J_1(5E_1) + 2H_5,$$

$$M_5 - N_5 = 0,$$

$$
\begin{aligned}
6(M_6+N_6) = \quad & 2H_1\{J_1(6E_1)[J_1(6E_4)-J_2(6E_2)] \\
& +J_1(6E_2)[J_2(6E_1)+J_1(6E_3)] \\
& -J_5(6E_1)-J_1(6E_5)-J_2(6E_1)J_1(6E_3)\} \\
& +2H_2\{J_4(6E_1)+J_2(6E_2)-J_4(6E_4)+J_1(6E_1)J_1(6E_3) \\
& -J_1(6E_2)J_2(6E_1)\} \\
& +2H_3\{J_1(6E_1)J_1(6E_2)-J_2(6E_1)-J_1(6E_2)\} \\
& +2H_4\{J_2(6E_1)-J_1(6E_2)\} \\
& -2H_5J_1(6E_1) \\
& +2H_6,
\end{aligned}
$$

$$M_6-N_6=0\ . \qquad (20).$$

THE NUMERICAL VALUES OF THE COEFFICIENTS IN THE CASE OF THE FREE SURFACE.

From the conditions (7) which we have found for the free surface we may find, from (8), the corresponding values of the E's (which I shall write in small type when $b=0$) in terms of h_1. I shall also omit the suffix from h_1, since this is now the only one of the set retained.

These values are found to be

$$
\begin{aligned}
e_1 &= 2h+2h^3+10h^5, \\
e_2 &= 2h^2+\tfrac{7}{2}h^4+\tfrac{253}{12}h^6, \\
e_3 &= 3h^3+\tfrac{131}{15}h^5, \\
e_4 &= \tfrac{106}{3}h^4+\tfrac{2356}{144}h^6, \\
e_5 &= \tfrac{1255}{12}h^5, \\
e_6 &= \tfrac{1015}{5}h^6 \ldots \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)
\end{aligned}
$$

Also, we shall have

$$
\begin{aligned}
J_0(re_1) &= 1-r^2h^2-\left(2r^2-\frac{r^4}{4}\right)h^4-\left(11r^2-r^4+\frac{r^6}{36}\right)h^6, \\
J_0(re_2) &= 1 \qquad\qquad -r^2h^4-\frac{7r^2}{2}h^6, \\
J_0(re_3) &= 1 \qquad\qquad\qquad -\frac{9r^2}{4}h^6\ ;
\end{aligned}
$$

$$J_1(re_1) = r\left(h - \left(\frac{r^2}{2} - 1\right)h^3 + \left(5 - \frac{3r^2}{2} + \frac{r^4}{12}\right)h^5\right),$$

$$J_1(re_2) = r\left(h^3 + \frac{7}{4}h^4 + \left(\frac{233}{24} - \frac{r^2}{2}\right)h^6\right),$$

$$J_1(re_3) = r\left(\frac{3}{2}h^3 + \frac{131}{36}h^5\right),$$

$$J_1(re_4) = r\left(\frac{8}{3}h^4 + \frac{2359}{285}h^6\right),$$

$$J_1(re_5) = r\frac{125}{24}h^5,$$

$$J_1(re_6) = r\frac{54}{5}h^6;$$

$$J_2(re_1) = \frac{r^2}{2}\left(h^2 - \left(\frac{r^2}{3} - 2\right)h^4 + \left(11 - \frac{4r^2}{3} + \frac{r^4}{24}\right)h^6\right),$$

$$J_2(re_2) = \frac{r^2}{2}\left(h^4 + \frac{7}{2}h^6\right),$$

$$J_2(re_3) = \frac{9r^2}{8}h^6;$$

$$J_3(re_1) = \frac{r^3}{6}\left(h^3 - \left(\frac{r^2}{4} - 3\right)h^5\right),$$

$$J_3(re_2) = \frac{r^3}{6}h^6;$$

$$J_4(re_1) = \frac{r^4}{24}\left(h^4 - \left(\frac{r^2}{5} - 4\right)h^6\right);$$

$$J_5(re_1) = \frac{r^5}{5!}h^5;$$

$$J_6(re_1) = \frac{r_6}{6!}h^6 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22).$$

All of these have been carried to the 6th order

The result of the substitution in the formulæ of the last section gives

$$A_1 = -2h - 3h^3 - \tfrac{29}{2}h^5,$$

$$M_1 = h + \tfrac{1}{2}h^3 + \tfrac{5}{4}h^5,$$

$$N_1 = h + \tfrac{3}{2}h^3 + \tfrac{27}{4}h^5;$$

$$\begin{aligned} A_2 &= -\tfrac{1}{6}h^4 + \tfrac{97}{360}h^6, \\ M &= -\tfrac{1}{6}h^4 + \tfrac{85}{360}h^6, \\ N &= \tfrac{1}{6}h^4 - \tfrac{97}{360}h^6; \end{aligned}$$

$$\begin{aligned} A_3 &= -\tfrac{1}{360}h^6, \\ M_3 &= -\tfrac{1}{24}h^6, \\ N_3 &= \tfrac{1}{24}h^5; \end{aligned}$$

$$\begin{aligned} A_4 &= -\tfrac{1}{2520}h^6, \\ M_4 &= -\tfrac{1}{120}h^6, \\ N_4 &= \tfrac{1}{120}h^6. \end{aligned}$$

$$A_5 = M_5 = N_5 = A_6 = M_6 = N_6 = 0 \quad . \quad (23).$$

Indicating by the suffix o that the symbols refer to the free surface, we have now obtained these values correct to the proposed degree of approximation:

$$ku_o = \phi_o = \mu_o - (2h + 3h^3 + \tfrac{20}{2}h^5)\sin\mu_o - (\tfrac{1}{6}h^4 - \tfrac{97}{360}h^6)\sin 2\mu_o - \tfrac{1}{360}h^5\sin 3\mu_o - \tfrac{1}{2520}h^6\sin 4\mu_o \quad . \quad . \quad (24).$$

$$kx_o = \mu_o - (h + \tfrac{5}{2}h^3 + \tfrac{53}{4}h^5)\sin\mu_o - (\tfrac{1}{3}h^4 - \tfrac{91}{15}h^6)\sin 2\mu_o - \tfrac{5}{72}h^5\sin 3\mu_o - \tfrac{1}{840}h^6\sin 4\mu_o \quad . \quad . \quad (25).$$

$$ky_o = -(h + \tfrac{3}{2}h^3 + \tfrac{27}{4}h^5)\cos\mu_o - (\tfrac{1}{6}h^4 - \tfrac{97}{360}h^6)\cos 2\mu_o - \tfrac{1}{24}h^5\cos 3\mu_o - \tfrac{1}{1120}h^6\cos 4\mu_o \quad . \quad . \quad (26).$$

where, from (8),

$$\mu_o = k(ct + a)/(1 + h^2 + 4h^4 + \tfrac{97}{4}h^6),$$

We also have the relation, from the surface-conditions in (7), that

$$kc^2/g = 1 + h^2 + \tfrac{7}{2}h^4 + \tfrac{229}{12}h^6,$$

and we can readily verify that the surface-condition, in the form that $2gy_o - cu_o$ shall be constant, is satisfied to the degree of our approximation.

The noteworthy features which the results just obtained present to us are their striking simplicity as

compared with the cumbrous expressions from which they have been derived, and also the rapid fall in the leading coefficients in the implicit equation to the wave-form contained in (25) and (26) after the first trigonometrical term, and the close approximation of that wave-form to a trochoid.

The equations also indicate that for a closer knowledge of the form of finite waves, we need more terms in the coefficients of the earlier trigonometrical terms rather than a greater number of terms. On this account no diagrams of the wave profile have been drawn.

The Wave-Profile.

Returning to equations (25) and (26), which give the implicit equations of the wave-profile, we will now write

$$\frac{2\pi}{\lambda} = k,$$

and

$$\frac{2\pi}{\lambda_0} = \frac{k}{1 + k^2 + 4k^4 + \frac{97}{4}k^6}$$

so that

$$\frac{\lambda}{\lambda_0} = 1 + k^2 + 4k^4 + \frac{97}{4}k^6$$

Then the equations may be written

$$x = \frac{\lambda}{\lambda_0}(ct + a)$$

$$- \frac{\lambda}{2\pi}\left(k + \frac{5}{2}k^3 + \frac{53}{4}k^5\right)\sin\frac{2\pi}{\lambda_0}(ct + a)$$

$$- \frac{\lambda}{2\pi}\left(\frac{1}{3}k^4 - \frac{91}{18}k^6\right)\sin\frac{4\pi}{\lambda_0}(ct + a)$$

$$- \frac{\lambda}{2\pi}\cdot\frac{5}{72}k^5\sin\frac{6\pi}{\lambda_0}(ct + a)$$

$$- \frac{\lambda}{2\pi}\cdot\frac{1}{80}k^6\sin\frac{8\pi}{\lambda_0}(ct + a),$$

$$y = -\frac{\lambda}{2\pi}\left(h + \frac{3}{2}h^3 + \frac{27}{4}h^5\right)\cos\frac{2\pi}{\lambda_0}(ct+a)$$
$$-\frac{\lambda}{2\pi}\left(\frac{1}{6}h^4 - \frac{97}{36}h^6\right)\cos\frac{4\pi}{\lambda_0}(ct+a)$$
$$-\frac{\lambda}{2\pi}\cdot\frac{1}{24}h^5\cos\frac{6\pi}{\lambda_0}(ct+a)$$
$$-\frac{\lambda}{2\pi}\cdot\frac{1}{120}h^6\cos\frac{8\pi}{\lambda_0}(ct+a). \quad . \quad . \quad (27)$$

Hence the horizontal distance apart of two particles indicated by (a,o) and $(a+\lambda_0,o)$ at any instant is λ, so that in the motion the wave-length (not only at the surface but throughout the motion) is λ. At a depth b the particles will be indicated by (a,b) and $(a+\lambda_b,b)$, where the value of λ_b is readily expressed from (8)

The motion of the particles is given in the next section, but the general character of the motion is illustrated by replacing λ_0 by λ_b in the equations just written and remembering that the coefficients will die away exponentially. It will be noticed that $\lambda = \lambda_\infty$ with this notation.

We have for the height of the wave

$$a = \frac{\lambda}{2\pi}\left(h + \frac{3}{2}h^3 + \frac{163}{24}h^5\right),$$

which on reversal becomes

$$h = \frac{2\pi a}{\lambda} - \frac{3}{2}\left(\frac{2\pi a}{\lambda}\right)^3 - \frac{1}{24}\left(\frac{2\pi a}{\lambda}\right)^5,$$

and this might appear a convenient form for substitution in the results just obtained. It can be shewn, however, that this reversal is only justifiable for small values of h, and in fact for values of h less than that value which will be determined later as the limit of the range of h.

The general solution of Lagrange's equations for the motion of a particle within the fluid mass is not of great importance, but, as the necessary calculations are pre-

pared, in order to complete the solution, I shall state the results.

With the object of making use of the simplifications already found, I consider the actual motion as obtained by means of a correction on a hypothetical motion dying away from the surface motion by the exponential law resulting from the substitution in the surface motion of H(*i.e.*, he^{-hz}) in place of h, the correcting terms being of the type $H^2(h^2-H^2)$, etc.

For example, from (7)

$$H_2 = H^2(2+h^2+\tfrac{29}{6}h^4),$$

and I consider this as

$$2H^2 + H^4 + \tfrac{29}{6}H^6 + (H^2 + \tfrac{29}{3}H^4)(h^2 - H^2) + \tfrac{29}{6}H^2(h^2 - H^2)^2,$$

and similarly with the other quantities.

By this method the chief terms (using the word "chief" not in a physical sense, but in reference only to the analytical method) in the values of A, M, N, are deduced from values derived from those of (23) by substituting H in place of h throughout.

The correcting terms are readily calculated from (17) and (20), and are

$$A_1 = \ldots - 3H^3(h^2 - H^2),$$
$$M_1 = \ldots + \tfrac{1}{2}H^3(h^2 - H^2),$$
$$N_1 = \ldots + \tfrac{3}{2}H^3(h^2 - H^2),$$

$$A_2 = \ldots - H^2(h^2 - H^2) - \tfrac{63}{4}H^4(h^2 - H^2) - \tfrac{29}{6}H^2(h^2 - H^2)^2,$$
$$M_2 = \ldots + \tfrac{1}{2}H^2(h^2 - H^2) + 5H^4(h^2 - H^2) + \tfrac{29}{12}H^2(h^2 - H^2)^2,$$
$$N_2 = \ldots + \tfrac{1}{2}H^2(h^2 - H^2) + \tfrac{37}{6}H^4(h^2 - H^2) + \tfrac{29}{12}H^2(h^2 - H^2)^2,$$

$$A_3 = \ldots - \tfrac{1}{6}H^3(h^2 - H^2),$$
$$M_3 = \ldots + \tfrac{1}{12}H^3(h^2 - H^2),$$
$$N_3 = \ldots + \tfrac{1}{12}H^3(h^2 - H^2),$$

$$A_4 = \ldots - \tfrac{1}{36}H^4(h^2 - H^2),$$
$$M_4 = \ldots + \tfrac{1}{72}H^4(h^2 - H^2),$$
$$N_4 = \ldots + \tfrac{1}{72}H^4(h^2 - H^2).$$

From these we obtain, writing he^{-kb} for H, as the equations corresponding to (24), (25), and (26) :—

$$ku = \mu - \{\tfrac{1}{2}he^{-kb} + 3h^3e^{-3kb} + h^5(3e^{-3kb} + \tfrac{27}{32}e^{-5kb})\} \sin\mu$$
$$- \{h^4(e^{-2kb} - \tfrac{5}{6}e^{-4kb}) + h^6(\tfrac{27}{24}e^{-2kb} + \tfrac{71}{12}e^{-4kb} - \tfrac{245}{192}e^{-6kb})\}\sin 2\mu$$
$$+ \tfrac{1}{3}\tfrac{3}{2}h^3(5e^{-3kb} - 6e^{-5kb}) \sin 3\mu - \tfrac{1}{12}\tfrac{3}{80}h^4(20e^{-4kb} - 17e^{-6kb}) \sin 4\mu$$

$$kx = \mu - \{he^{-kb} + \tfrac{5}{32}h^3e^{-3kb} + h^5(\tfrac{5}{32}e^{-3kb} + \tfrac{43}{64}e^{-5kb})\} \sin\mu$$
$$- \{h^4(\tfrac{1}{2}e^{-2kb} - \tfrac{1}{6}e^{-4kb}) + h^6(\tfrac{27}{128}e^{-2kb} + \tfrac{71}{128}e^{-4kb} - \tfrac{2461}{1536}e^{-6kb})\} \sin 2\mu$$
$$- \tfrac{1}{12}\tfrac{3}{32}h^3(6e^{-3kb} - e^{-5kb}) \sin 3\mu - \tfrac{1}{12}\tfrac{3}{80}h^4(10e^{-4kb} - e^{-6kb}) \sin 4\mu$$

$$ky = kb - \{he^{-kb} + \tfrac{3}{32}h^3e^{-3kb} + h^5(\tfrac{3}{32}e^{-3kb} + \tfrac{21}{64}e^{-5kb})\} \cos\mu$$
$$- \{h^4(\tfrac{1}{2}e^{-2kb} - \tfrac{1}{3}e^{-4kb}) + h^6(\tfrac{27}{128}e^{-2kb} + \tfrac{4}{3}e^{-4kb} - \tfrac{55}{96}e^{-6kb})\} \cos 2\mu$$
$$- \tfrac{1}{32}\tfrac{1}{4}h^3(2e^{-3kb} - e^{-5kb}) \cos 3\mu - \tfrac{1}{32}\tfrac{1}{40}h^4(5e^{-4kb} - 2e^{-6kb}) \cos 4\mu. \qquad (28).$$

From these we may evaluate $2gy - cu$, to determine the law of variation of the pressure downwards, and so measure the error in Gerstner's assumption that the pressure is uniform along each stream line. The expression is, however, very cumbrous, as might be expected.

The Approximate Determination of the Extreme Value Permissible for h.

When the values of h_2, h_3,... from (7) are substituted in $f'(u + ib)$, the successive terms in the expansion have the general appearance of those of a divergent series. If, however, attention is confined to the leading terms in the coefficients in the series, it may be noticed that there is a marked resemblance, in point of divergence, with those of the Binomial expansion of $(1 - 3he^{ikx})^{-\frac{1}{3}}$. In order to bring into prominence and to test the extent of this

resemblance, I write $f'(u+ib)$, when $b=0$, in the form $\phi(iku)$, and work out the value of $\{\phi(iku)\}^{-3}$ on the assumption of convergence.

I thus find—

$$\{\phi(iku)\}^{-3}$$
$$= 1 - 3he^{iku} - 3(h^4 + \tfrac{20}{6}h^6)e^{2iku} + (\tfrac{1}{2}h^3 - \tfrac{9}{4}h^5)e^{3iku}$$
$$+ (h^4 - \tfrac{7}{6}h^6)e^{4iku} + \tfrac{15}{8}h^5e^{5iku} + \tfrac{18}{5}h^6e^{6iku} + \ldots \qquad (29)$$

In this the reduction of the magnitude of the later coefficients is very noteworthy. And, although a continuation of the series in the coefficients is desirable, it would appear that under proper conditions the right-hand side is rapidly convergent and that the series can be properly reversed.

To examine the necessary conditions more fully, it becomes manifest that the expression on the right-hand side of (29) will have a factor of the form $(1-\zeta e^{iku})$ where ζ is not widely different from $3h$, and to which we may approximate more closely.

Thus, when we write

$$3h = \alpha\zeta + \beta\zeta^3 + \gamma\zeta^5,$$

we may determine α, β and γ approximately so as to satisfy the condition

$$1 - \frac{3h}{\mu} - \frac{3(h^4 + \frac{20}{6}h^6)}{\mu^2} + \ldots\ldots = 0$$

from (29), with some amount of accuracy. The results will only give an approximation to the actual values, by the nature of the method used.

The equations obtained are:

$$1 - \alpha + \tfrac{1}{14}\alpha^2 + \tfrac{1}{81}\alpha^4 + \tfrac{5}{448}\alpha^5 + \tfrac{2}{105}\alpha^6 = 0,$$
$$\beta(1 - \tfrac{1}{15}\alpha^3 - \tfrac{4}{81}\alpha^4) = -(\tfrac{1}{27}\alpha^4 + \tfrac{1}{108}\alpha^5 + \tfrac{7}{756}\alpha^6),$$
$$\gamma = -(\tfrac{4}{27}\alpha^3\beta + \tfrac{20}{189}\alpha^6) \quad . \quad . \quad . \quad (30).$$

These give approximately

$$\begin{aligned} \alpha &= \quad 1{\cdot}054, \\ \beta &= -\ {\cdot}068, \\ \gamma &= -\ {\cdot}016 \end{aligned} \qquad . \quad . \quad . \quad (31),$$

and these lead to

$$h = {\cdot}351\zeta - {\cdot}023\zeta^3 - {\cdot}005\zeta^5 \qquad . \quad . \quad (32).$$

This value increases with increasing fractional values of ζ, and reaches the value ·323 when ζ reaches the value unity.

Since the hypothesis of convergency would certainly fail when ζ exceeds unity, we may take this to give approximately the upper limit of the range of values permissible for h.

With these numbers we also find from (7)

$$\frac{kc^2}{g} = 1 + {\cdot}123\zeta^2 + {\cdot}017\zeta^4 + {\cdot}019\zeta^6, \quad . \qquad . \quad (33).$$

and

$$ka = {\cdot}351\zeta + {\cdot}041\zeta^3 + {\cdot}019\zeta^5$$

where $2a$ is the height of the wave from trough to crest, and

$$\frac{2a}{\lambda} = {\cdot}112\zeta + {\cdot}013\zeta^3 + {\cdot}006\zeta^5, \qquad . \quad (34)$$

if λ is the distance from crest to crest of the wave.

This gives for the greatest value of this ratio

$$\frac{2a}{\lambda} = {\cdot}131$$

approximately, a value not very greatly in defect of that, viz., ·142, found by Mr. Mitchell for the pointed wave.

With the same numerical values I find

$$\begin{aligned} &\{\phi(iku)\}^{-1} \\ &= (1 - \zeta e^{iku})^{\frac{1}{2}}\{1 - ({\cdot}018\zeta - {\cdot}023\zeta^3 - {\cdot}005\zeta^5)e^{iku} \\ &- ({\cdot}018\zeta^2 - {\cdot}009\zeta^4)e^{2iku} - ({\cdot}011\zeta^3 - {\cdot}003\zeta^5)e^{3iku} \\ &- {\cdot}006\zeta^4 e^{4iku} - {\cdot}003\zeta^5 e^{5iku} \qquad . \quad . \quad . \quad . \quad (35) \end{aligned}$$

If we introduce ζ in place of h in the equations (27) to the wave profile, and omit terms in which the coefficient is less than ·001, we get

$$x = \frac{\lambda}{\lambda_0}(ct+a)$$

$$-\frac{\lambda}{2\pi}(\cdot 351\zeta + \cdot 091\zeta^3 + \cdot 047\zeta^5)\sin\frac{2\pi}{\lambda_0}(ct+a),$$

$$-\frac{\lambda}{2\pi}(\cdot 005\zeta^4 - \cdot 011\zeta^6)\sin\frac{4\pi}{\lambda_0}(ct+a),$$

$$y = -\frac{\lambda}{2\pi}(\cdot 351\zeta + \cdot 042\zeta^3 + \cdot 018\zeta^5)\cos\frac{2\pi}{\lambda_0}(ct+a)$$

$$-\frac{\lambda}{2\pi}(\cdot 003\zeta^4 - \cdot 006\zeta^6)\cos\frac{4\pi}{\lambda}(ct+a),$$

where $$\frac{\lambda_0}{\lambda} = 1 + \cdot 123\zeta^2 + \cdot 045\zeta^4 + \cdot 028\zeta^6 \quad . \quad . \quad (36).$$

From these it will be seen that although it is needless to obtain further terms in the trigonometrical series, the continuation of the coefficients of the first two trigonometrical terms would be necessary in order to draw diagrams, except for moderate values of ζ.

The Connection with Mr. Mitchell's Highest Wave in Water.

The results of the last section suggest a change in the method of treating the equation of surface condition in order to determine the coefficients in the expansion of the velocity more conveniently, and also to justify more fully the convergency of the expressions obtained. It is

also proposed to find a connection with the process employed by Mr. Mitchell.

For this purpose, I differentiate with respect to u, the equation of surface condition (3), and obtain

$$\frac{g}{i}\left\{\phi(iku)-\phi(-iku)\right\}=c^2\frac{d}{du}\left\{\phi(iku)\phi(-iku)\right\}^{-1} \quad . \quad . \quad (37).$$

In the first case, I write

$$\phi(iku)=\{\psi(iku)\}^{-\frac{1}{2}},$$

and obtain

$$-\frac{g}{i}\left[\{\psi(iku)\}^{\frac{1}{2}}-\{\psi(-iku)\}^{\frac{1}{2}}\right]$$

$$=\frac{c^2}{2}\frac{d}{du}\{\psi(iku)\psi(-iku)\}^{\frac{1}{2}} \quad . \quad . \quad (38).$$

On rationalising the left-hand side of this equation, the right-hand side will be found also to have become rational. Thus

$$\frac{g^3}{i}\left[\psi(iku)-\psi(-iku)\right]=\frac{c^6}{8}\left[\frac{d}{du}\{\psi(iku)\psi(-iku)\}^{\frac{1}{2}}\right]^3$$

$$-c^2g^2\frac{d}{du}\left\{\psi(iku)\psi(-iku)\right\} \quad . \quad . \quad (39).$$

It would appear that a continuation of the terms in (29) could be most readily obtained from the condition in this form.

The stage most consonant with the final form of the result will be reached by writing

$$\left.\begin{aligned}\phi(iku)&=(1-\tfrac{1}{2}e^{iku})^{-\frac{1}{2}}U\\ \text{and}\quad \phi(-iku)&=(1-\tfrac{1}{2}e^{-iku})^{-\frac{1}{2}}V\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (40).$$

where U and V are rapidly converging series of the types

$$1 + C_1 e^{iku} + C_2 e^{2iku} + \ldots$$

and

$$1 + C_1 e^{-iku} + C_2 e^{-2iku} + \ldots$$

In this case the velocity $(qe^{i\omega})$ is given by

$$qe^{i\omega} = C(1 - \zeta e^{iku})^{\frac{1}{2}} U$$

and

$$e^{2i\omega} = \frac{(1-\zeta e^{-iku})^{\frac{1}{2}}}{(1-\zeta e^{iku})^{\frac{1}{2}}} \cdot \frac{V}{U}$$

In this case the point of inflexion in the surface approaches the crest continually as ζ approaches, but is finitely less than, unity,* and the character of the singularity at the crest when $\zeta = 1$ is clearly indicated by the formula.

We may now obtain the expression employed by Mr. Mitchell. For

$$\frac{g}{i}\left\{\frac{1}{(1-\zeta e^{iku})^{\frac{1}{2}} U} - \frac{1}{(1-\zeta e^{-iku})^{\frac{1}{2}} U}\right\}$$

$$= e^{\frac{1}{2}} \frac{d}{du}\left\{(1-\zeta e^{iku})^{\frac{1}{2}}(1-\zeta e^{-iku})^{\frac{1}{2}} U^{-1}\right\}$$

$$= e^{\frac{1}{2}}\left\{(1-\zeta e^{iku})^{\frac{1}{2}}(1-\zeta e^{-iku})^{\frac{1}{2}}\right\} \frac{d(U^{-1})}{du}$$

$$+ \frac{2k\zeta}{3} \frac{\sin ku}{(1-\zeta e^{iku})^{\frac{1}{2}}(1-\zeta e^{-iku})^{\frac{1}{2}}} U^{-1}\Big\}$$

may be written

$$\frac{g}{i}\left\{\left(\frac{1-\zeta e^{iku}}{1-\zeta e^{-iku}}\right)^{\frac{1}{2}} U - \left(\frac{1-\zeta e^{-iku}}{1-\zeta e^{iku}}\right)^{\frac{1}{2}} U^{-1}\right\}$$

$$= e^{\frac{1}{2}}\left\{\frac{(1-\zeta e^{iku})^{\frac{1}{2}}(1-\zeta e^{-iku})^{\frac{1}{2}}}{2} \frac{d}{du}(U^2 U^{-2})\right.$$

$$\left. + \frac{2k\zeta}{3} \frac{\sin ku}{(1-\zeta e^{iku})(1-\zeta e^{-iku})^{\frac{1}{2}}} U^{-1}\right\}. \quad (41).$$

* To a first approximation the position of the point of inflexion is given by $\cos ku = \zeta$.

This becomes, in the special case when $\zeta = 1$, identical, except for the difference in units, with the equation which Mr. Mitchell has employed to obtain the circumstances of the Highest Wave in Water.

We may, however, proceed to rationalise the expressions as in the previous case, and thus obtain

$$\frac{g^3}{i}\left\{(1-\zeta e^{iku})U^3-(1-\zeta e^{-iku})V^3\right\}$$

$$=\frac{c^6}{8}\left[\frac{d}{du}\left\{(1-\zeta e^{iku})^{\frac{1}{2}}(1-\zeta e^{-iku})^{\frac{1}{2}}U^2V^2\right\}\right]'$$

$$-c^2g^3\frac{d}{du}\left\{(1-\zeta e^{iku})(1-\zeta e^{-iku})U^2V^2\right\}. \quad (44).$$

This may be used when ζ is less than unity to determine the coefficients to the expansions of U and V, and also, when $\zeta = 1$, after division by $(1-\zeta e^{iku})^{\frac{1}{2}}(1-\zeta e^{-iku})^{\frac{1}{2}}$, it may be conveniently made use of for the same purpose, since the series on both sides would proceed in cosines of multiples of the angle $ku/2$.

By the theory now proposed, the form of the solution is the same whether for rounded or for the critical pointed wave. In both cases the solution shows poles, which in the case of critical pointed waves lie on the surface of the fluid, but in the case of the rounded waves are situated above the surface at a less or greater distance according to the height of the waves.

It has not been shown that the equation of surface condition has a solution of this special form, but merely that the terms of the solution in series found by another method submit to a great simplification when treated in a special way.

This being the case, a conclusion in regard to waves under other circumstances would not be justified, but it

would seem not unlikely that the solutions in the case of other irrotational waves are susceptible of similar simplification.

The Practical Limit of the Height of Waves.

In this paper I have not considered either the origination of the wave, nor its propagation into still water, but only the circumstances under which uniform propagation is possible; and a differential motion of drift is one such circumstance. The magnitude of this drift of the fluid which diminishes from the surface downward is worth a closer consideration, for it appears probable that the practical limit of the height of deep water waves is effectively determined by this magnitude and not by the theoretical limit to the extreme shape of the wave.

When the steady motion with which we have started is reduced to a progressive wave motion, the measure of the rate of this drift at the water surface is, by (27),

$$\left(1 - \frac{\lambda}{\lambda_0}\right)c$$

or, on simplification,

$$(h^2 + 3h^4 + \tfrac{7}{4}h^6)c \qquad (44).$$

If we give to h the extreme value ·323 which we have approximately assigned to it, this will turn out to be about ·15c, or nearly one-sixth of the velocity of the wave. If we give h the value ·25, the rate of drift falls to ·07c about, that is to less than one-half of its value in the theoretically extreme case.

After allowing for the fact that the series used in (44)

have not been proved to remain convergent for the range of values given to h, it will still remain that the amount of drift necessary increases very rapidly with the height of the wave, and that this will be an important factor, if not the predominant factor, in limiting the height of such waves in nature.

The same conclusion will follow from the consideration of the horizontal momentum of the fluid per wave-length which will, to a first approximation, vary as the square of the height of the wave, and, including the further approximate terms, at a more rapid rate than this.

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IX. Observations on a Captive Mole (*Talpa europæa*).

By LIONEL E. ADAMS, B.A.

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The following extracts from my field note book may be of interest, as they touch upon some of the many mole problems yet unsolved:—

At noon, on December 15th, 1904, I found a mole on a path in Reigate Park. The little beast was wandering aimlessly about without attempting to burrow. I brought him home in one of my leather gloves—not in my pocket for reasons which previous experience had furnished—and placed him in an empty sugar case with six inches of earth banked up at one end. During the afternoon I noticed him shivering as if with cold, and I provided him with a large handful of hay rolled up to resemble a mole's nest, placing it on the top of the earth in his box. He soon came upon it, went inside and lay perfectly still; I believe he went to sleep. I fed him with worms at intervals during the evening, and next morning found him active and hearty, hurrying about in search of food. That he had survived the night with only a few worms left was doubtless due to the fact that he was enabled to keep warm in the hay. I do not know whether it has ever occurred to anyone to provide a nest for a captive mole, but this is doubtless necessary to sustain the animal's heat, especially when there is no great depth of soil for burrowing; and the constant failures to keep a mole alive in captivity are due to the want of this quite as much as to the alleged lack of sufficient food. Now, I kept this

mole for eight days, and turned him out at last as hearty and vigorous as when I first caught him, and this without leaving nearly as much food as I have done on previous occasions when the captives succumbed during the night.

I noticed one day, when the weather was somewhat colder, that he made the nest more compact by pulling in the hay from the inside. On the fourth day he became very restless, so I changed all the earth in the box as well as the hay, thinking that the dead worms and mice with which I had supplied him had fouled his restricted quarters, though I could detect no offensive odour attending him; but he continued restless till I gave him his liberty on the eighth day of his captivity, placing him in a neighbouring field where he made many mounds, and I finally lost sight of him when the field was ploughed.

Writing in the *Ulster Echo*, July 31st, 1903, Mr. M. Woodward, the author of an article on the mole in *Pearson's Magazine* for July of that year, quoting Mr. A. Runciman, says: "[The mole] having caught his worm . . . seizes it by the tail, off which he bites a small piece; then he turns the worm round. This is accomplished with his paws, the sides of which, while their palms turn towards the front, grip the worm firmly. Then the mole, having turned the worm, draws it into his mouth with a series of short quick jerks, at the same time moving his paws slightly forward. And the effect of this movement is to cause the earth to squirt out at the tail end, the tip having been cut off purposely to give the earth free vent. Thus the mole secures a clean meal, without any distasteful clay. Evidently he knows that the tail is the proper part of the worm to bite off, and that he must begin feeding at the nose to effect his purpose."

Now, during the time that my captive was under close observation—indeed I spent most of my time in his

ompany—I watched to see him adopt this method of feeding, but in vain. I never once saw him bite off the tail of a worm or turn it round. My mole after seizing a worm with its mouth would hold it down with his paws and feel the way with his snout to one end (as often the head as the tail), and would eat the worm from that end to the other in exactly the manner that Mr. Runciman has described—by a series of short quick jerks. I have, however, noticed now and then that the mole would brush away the external earth from the worm with his snout and paws before commencing to devour it. That the mole is particular about the earthy contents of a worm is doubtful, as I have as often as not found the stomachs of moles full of earthy matter. Some moles, nevertheless, may be more fastidious and scour their worms, but I have never seen this done by any of the many moles I have fed and watched.

On one occasion a large lobworm had burrowed nearly out of sight, when the mole came upon it and seized it, but instead of tugging at it furiously, as I had expected, and thereby breaking it, he held it taut, and presently yielding to the gentle tension, it was secured whole. This knowledge of the fragile nature of a worm, to say nothing of the ultimate consequences of haste and fracture, is remarkable, and the self-restraint on the part of so impetuous a creature is still more so.

On one occasion, when I had fed him till he could eat no more, he took a worm, bit it with quick bites along its whole length, and then crammed it into the earth, left it, and turned about to find another. I gave him another, a large lobworm, which he treated in precisely the same manner, thrusting it into the same hole, which he straightway covered up by scraping the earth over it with his fore paws. On two other occasions I watched him bury

worms, and once a dead mouse, in the same way. I had never previously seen moles bury worms, and it is interesting, as it tends to confirm the old stories of moles storing up worms for food.*

Worms are undoubtedly the favourite food, though my captive ate slugs without hesitation (*Amalia sowerbyi*, *Arion hortensis*, and *Agriolimax agrestis*).

I offered him also freshly killed bank voles and long-tailed field mice, but he would only tackle them when worms failed, and then only when slit open. The heart, lungs, and intestines were the only parts touched. Respecting the mole's food, the Rev. A. Woodruffe-Peacock writes: "I have known young birds held by the feet by moles in the shallow runs, a blackbird and young pheasants .... Within the last four winters my brother and I found a hooded crow picked clean by a mole in the middle of a meadow. The frozen snow and tracks to its earth proved it to be a mole which had fed on the dead bird."

The more I watch moles, the more convinced I am that they are practically blind. When a worm is placed near a mole the latter immediately shows signs of excitement, being aware of its proximity by smell or hearing, but it is only after poking about with its snout that it strikes it haphazard. The mole never goes straight for the worm, and when a half-eaten worm is dropt and has crawled a little way, the same hunt for it is repeated.

I kept the captive in a greenhouse, and one evening he escaped by climbing up the rough corner of his box

* Shortly after writing the above, I met a farmer on whose land I study moles, and he informed me that a few days previously (March 30) while digging out a fortress on one of his meadows he came upon a heap of dead worms in the nest cavity close beside the nest. He described it as "three spadefuls." This is most interesting, and is a perfectly reliable confirmation of the stories of stored worms.

and played havoc among the borders against the walls, raising mounds and uprooting plants. I took a candle to aid his recapture, and placed it on the ground just where he was about to emerge. Upon coming to the surface he paid no attention to the candle in front of him, and blundered into the candlestick, but directly I stirred to seize him he was off again. I took the candle and watched him burrow and emerge in another place; I waved the candle in front of him, but he took no notice. This experiment with a lighted candle I have repeatedly tried with captive moles at different times, and invariably with the same result, and I cannot avoid the conclusion that moles are practically blind, for surely no animal would be indifferent to such a startling and unfamiliar phenomenon as a light waving suddenly close in front of its eyes, unless, indeed, the light was imperceptible. When I had caught and replaced him I fastened some wire netting over his box. The next morning I found him actually hanging to the wire netting with his front paws, poking his nose through the meshes, and travelling from one hole to another with his body hanging perpendicularly. I regretted the light was not good enough to obtain a photograph of him in this unique position.

It is astonishing how soon moles become tame or rather indifferent to being handled. When first caught they squeak and bite viciously, but within an hour I have taken them up by the skin of the back without their protesting. I have frequently stroked and tickled my captives while they were engaged in eating, and I have held them up by the tail while they lapped water. I taught my last captive to come blundering along for food when I scratched upon the earth or the side of his box. This sudden tameness or indifference I have observed with long-tailed field mice, short-tailed field mice, and

young bank voles which I have come upon in hedge banks; by approaching them carefully I have been able to tickle them with a stick, and even to take them in my hand.

The folklore of the mole is well worth collecting. The Rev. A. Talbot, of Church Eaton, near Stafford, once told my friend, Mr. C. Oldham, and myself that three or four years ago an old mole catcher in the neighbouring village of High Onn assured him that an infallible cure for the toothache was to catch a mole, cut off its fore-feet, and then release the animal, which would carry away the pain! So long as the two feet were carried on the person, immunity from toothache was assured. The old man had a mole's fore-feet screwed up in paper in his pocket; these Mr. Talbot secured after his death. Mr. Oldham sends me the following extract from the report of a lecture by Dr. C. B. Plowright on Mediæval Medicine. "Among amulets in use in 1903 and 1904, a teething amulet formed of a piece of violet root (*Iris florentina*) and suspended by a tape to the babe, and a pair of mole's hands."

Pennant in his "History of Quadrupeds," vol. 2, p. 484, speaking of moles, says: "*Palma christi* and white *hellebore*, made into a paste and laid in the holes, destroys them."

Since my first paper on the present subject was published* I have been in correspondence with Mr. W. Evans, of Edinburgh, who has kindly presented me with a copy of his "Mammalian Fauna of the Edinburgh District" (Edinburgh, 1892), containing a most interesting article on the Mole, with a plan of a fortress by the

* "A Contribution to our Knowledge of the Mole (*Talpa europæa*)," vol. 47, part II., of the *Memoirs and Proceedings of the Manchester Literary and Philosophical Society*, Session 1902-1903.

author, who, of course, forestalls me in exploding the old legend of the fortresses being universally constructed as in the time-honoured figure.

When I was writing my former paper, I read all the literature on the subject that I could hear of, and I regret that neither of the above publications came under my notice. The only author that I was then aware of who cast a doubt on the legend was the hero of our boyhood, Captain Mayne Reid, in his "Naturalist in Siluria" London, 1889, a delightful book of *real* observations, which deserves to be better known

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X. A New Fern from the Coal Measures: *Tubicaulis Sutcliffii* spec. nov.

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Received and read May 8th, 1906.

1. INTRODUCTORY.

The "Age of Ferns" is the misleading name which has been sometimes applied to the period during which the Coal Measures were deposited. This name, which no longer represents the facts known to us, is now threatened with the fate of most of man's surmises, and the "Age of Pteridosperms" seems to be replacing it. Since the publication of the paper by Oliver and Scott (:04) on *Lagenostoma*, the seed of *Lyginodendron*, recent work on similar lines has rapidly accumulated numbers of important facts, and, as Scott (:05) demonstrated in the Wilde Lecture, we must conclude that many of the palæozoic "ferns" were not true pteridophytes. The great phylogenetic importance of these discoveries has led to an accession of interest in the plants of this period, which are of such assistance to botanists in filling the gaps in their knowledge of the "Natural System" aimed at since the time of Linnæus. The discovery of seeds on fern-like plants has also increased our curiosity regarding the actual ferns of the same period; but strange to say, in this 'Age of Ferns' the number of such plants with their structure preserved so that anatomical study is possible, is exceedingly limited. Though scattered fragments are of frequent occurrence, ferns are all too rare

which show both axis and branches or petioles associated so that a fairly complete idea of the plant is obtainable.

The discovery, therefore, in the Bullion mine at Shore, of a new fern with its main axis surrounded by numbers of petioles and roots, and of such a size that a series of sections could be cut from it, was an exceedingly welcome one. I am extremely indebted to W. H. Sutcliffe, Esq., F.G.S., of Shore, the owner of the mine, for his great generosity in placing in my hands the whole series of sections of this unique specimen for description in the interest of science. I have named it *Tubicaulis Sutcliffii*, in honour of the man who has done so much on many occasions for the cause of Palæobotany.

The plant is not only new to Britain, but so far as I am aware is the first of its genus to be found in the Coal Measures. The single other species of this rare genus has been found but once, and that in 1815, in the Middle Permian of the continent.

2. General Structure.

The specimen was about 4½ inches in length, and, including all the petioles preserved together, about 2 × 4½ inches in diameter. From this single block Mr. Lomax, of Bolton, succeeded in cutting about 60 sections in transverse and longitudinal directions, leaving a block uncut of about an inch in thickness. It seems to have been the upper end of the plant; and most of the transverse sections show the central main axis relatively small, surrounded by a large number of petioles of varying size, those just coming off being smaller than the main axis, and those further removed much exceeding it. Between these larger branches are innumerable small ones, of which the bulk are adventitious roots (see *Fig.* 2). Towards the upper end of the plant the differentiation of the tissues

becomes much less perfect, and the number of the petioles less, till finally in the last few sections the tissues of the main axis and neighbouring petioles appear to be extremely immature, and must lie near the growing point.

The general structure and appearance of the plant is that of a herbaceous fern. The distinctive character of the numerous petioles, which come off from the main axis as small branches and increase in size as they leave it, is only found in ferns that live in the ground. The general arrangement is like that of an *Aspidium* or *Athyrium* among living ferns, or an *Osmundites* among fossils. The rapid increase in size on the part of the petioles till they greatly exceed the main axis in diameter separates it further from most ferns, though this is also characteristic of some species of *Athyrium*. The form of the meristele, however, differs from any of these, as its simple horseshoe curve is orientated with the *convex* side toward the main axis, a feature of great rarity, which occurs only in *Asterochlaena dubia*, possibly in *Anachoropteris*, and in *Tubicaulis Solenites*.

The petioles apparently come off from the axis equally on all sides, and bend slowly away from it. The main axis itself appears to have been nearly vertical, but to have bent slightly. Large numbers of roots coming off from the axis and petioles branch there, and run in a most irregular fashion. One is led to believe that the plant was probably growing underground, the main axis being a nearly vertically running rhizome. The enormous number and the size of the petioles in comparison with that of the stem indicate that in the air the axis could not well have been self supporting, while their close arrangement and very short internodes preclude the idea of a climbing plant. The leaf bases apparently persisted for a long time, as there are many in the lowest section

(which is furthest from the growing region), most of which must have been attached some distance further back along the rhizome beyond the fragment now in our hands.

The internal anatomy is simple for so large a plant, there being but a single solid stele in the main axis, and a simple band of vascular tissue in the petiole, in neither are there definite sclerised bands, though the outer cortex is somewhat thickened.

The preservation throughout is of that curious type well known to palæobotanists as "roof preservation" (see p. 17), which affords local patches of tissue exquisitely fossilised with every detail retained, and other parts entirely wanting. From this cause, the work of reconstructing the plant has been extremely difficult, for in one section the axis bundle or whatever is being studied may be well preserved, while in the neighbouring sections of the series no trace of it may be seen. This partly accounts for the many instances in which it has been impossible to speak quite definitely about a point under discussion. Such difficulties are always encountered to a certain extent in fossil work, but in "roof nodules" of this type they are more serious than usual.

3. THE MAIN AXIS.

Although it is a little difficult to determine the exact limit of the cortex, the diameter of the main axis appears to be just short of one centimetre. Its external form is approximately cylindrical, broken at frequent intervals by the off-coming petioles. The external surface was apparently smooth, but the petioles are so closely arranged that but little of it would be exposed.

The stele is a single nearly circular one, of the simplest monostelic type. The *xylem* mass is absolutely

uniform and solid, and about 2 mm. across (see *Figs.* 3 and 4). The position of the protoxylems is not absolutely determined, partly owing to the preservation, partly to the fact that they appear not to have been very markedly differentiated from the rest of the tracheides. In transverse sections certain rather indefinite groups of smaller elements are found near to or at the edge of the stele, and from the uniform appearance of the central portions one may safely say that the bulk of the wood, if not all, was centripetally developed (see *Fig.* 3). In some cases such groups appear oblique, in transverse sections, and they show simple scalariform markings. A number of longitudinal sections were specially prepared, but in none did the preservation justify the trouble. Spiral and annular markings appear to be absent (or never preserved), not only in the axis, but also in both petioles and roots. This is not uncommon in fossil ferns, and may be a sign of slow growth; the small scalariform tracheides may well be the actual protoxylem elements. The tracheides of the main mass are on the whole very uniform, about 0·15 mm. in diameter, their corners fitting closely in together. The wood mass appears to be absolutely solid, as in none of the sections can I find any suggestion of soft inner tissue (as in *Zygopteris*, *Rachiopteris corrugata*, etc.) or of xylem parenchyma. The form of the tracheides is fern-like, many being simply scalariform, with one or two rows of elongated pits; the larger ones, however, show series of small round pits. The larger tracheides of this type show 6—8 rows of these pits (*cf. Fig.* 7), and are not paralleled, so far as I am aware, in any recent fern, though frequent in the Botryopterideae.

The *phloem* is not preserved in most of the sections, but obviously it would have lain in the space surrounding the xylem. In two sections in which the axis has locally

undergone "carbonisation" prior to its fossilisation, a zone of smallish thin walled cells is to be seen just outside the xylem. Of these, one or two rows have much the form of the rather large celled metaphloem, frequent in living ferns (see *Fig.* 5), and it is likely that they are the phloem in this case. *Pericycle* and *endodermis* are not preserved in any determinable form, but the presumption is natural that they surrounded the vascular elements as in the normal fern type.

The *cortex* is in general less well preserved than that of the petioles; there is a large space between the stele and those layers of the cortex which are preserved, which may represent a more delicate inner zone. The cortical layers present are composed of closely-fitting rounded cells with rather thickened walls and no air spaces. I have not been able to observe the *epidermis*.

There seems to be nothing peculiar or even characteristic about the outer zone of the cortex which might lead to the correllation of this plant with any other impressions or fragments, as neither zones of thickening, hairs nor emergencies of any sort are to be found on it.

Towards the upper end of the specimen the tissues become much less clearly differentiated, and finally, though still at least an inch from the actual apex, no xylem is recognisable. The other tissues are less thick-walled and the cells contain more small fragments which may represent cell contents, while many of them contain blackened contents and others numbers of small roundish bodies which may be starch grains (see p. 10 and compare *Fig.* 9). These appearances, combined with the smaller size and number of the petioles surrounding the main axis (these also appearing undifferentiated), lead to the conclusion that the apical growing point is approaching. The series of sections stops here because the preservation of the cen-

tral parts is so poor that there seemed no prospect of being able to see the actual growing point. There is a small block remaining, however, in which we can feel confident that the apex existed whether it is now preserved or not. The sections of this region reveal the fact that the parts attained a considerable size prior to the differentiation of the xylem, but they threw no light on the question of the position of the protoxylem or of the direction of differentiation of the metaxylem.

4. Branching.

The main axis, or rhizome, runs the length of the specimen without branching directly, or giving off any structure of the same order as itself. Surrounding the main axis are the numerous petioles which it gives off in rapid succession. In each transverse section there are several petioles closely associated with it, though it is impossible to be sure whether or not they have entirely separated from the main cortex, as the outer layers of both axis and petioles are wanting, of these, three are generally undoubtedly within the main axis cortex. At the time when the two cortices are certainly distinct the diameter of the petiole is slightly less than that of the main axis, and about one quarter that of the largest petiole preserved.

The *phyllotaxy* appears to be exceedingly near to, if not actually $\frac{2}{5}$, and the spiral is a close one. The reasons that there is any doubt about the matter are, (1) the rather uncertain preservation, (2) the fact that both axis and petioles bend, though but slightly, independently and at different angles. The plant with which it is natural to compare this, viz., *Tubicaulis Solenites*, is described by Stenzel ('89) as having a spiral of $\frac{13}{34}$ further out, and $\frac{8}{21}$ or $\frac{5}{13}$ near the axis, but in the case of my specimen, to

choose any such complicated fraction seems more than the conditions justify. In living plants it frequently happens that there is a slight twisting of the line of insertion which makes it difficult to determine the phyllotaxy from a small piece of the plant. In the case of a fossil, therefore, in which has been detected a slight bending of the axis, and of which there is at best but a few internodes from a single specimen, I prefer to choose the simplest common fraction which comes out nearly right, and therefore provisionally state the phyllotaxy to be $\frac{2}{5}$. (See *Text-fig.* 1.)

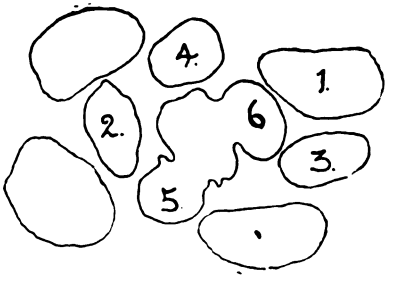

Fig. 1.—Plan of the petioles in a transverse section, shewing the phyllotaxy to be $\frac{2}{5}$. (*Slide* T 13.)

The **branching of the meristele** from the main axis is very simple, a group of tracheides (apparently including one or two of the supposed protoxylem elements) separates from the outer part of the main mass, and bends away from it (see *Figs.* 3 and 4). In transverse section this group appears oval or bean-shaped, gradually taking the form of a thick slightly bent arc as it leaves the stele. The *convex* side of the meristele is from the first directed towards the main axis. As it passes out the crescent form becomes more accentuated, and the ends begin to bend slightly round to form the **C** of the free petiole. In the

larger petioles the stele becomes very narrow, the C being somewhat laterally elongated and the ends more incurved, its orientation as regards the main axis, however, remains constant.

In many cases, as the simple group of tracheides is leaving the stele to pass out to the petiole, a smaller strand separates from the main axis and bends away from the stele to one side or other of the petiole strand. It runs out through the cortex and forms what I take to be an adventitious root (see *figs.* 3 and 4). It is soon lost among the crowds of other rootlets which bend and branch among the bases of the petioles.

Of anything corresponding to a true "axillary shoot" such as is found in *Zygopteris* and *Hymenophyllum*, there appears to be no trace.

How soon the main petioles branched to secondary ones it is not very easy to say, certainly it could not have been very soon after leaving the main axis, as I can find no case of the petiole stele branching to give off pinnules, and in two cases only can I find branch steles within the big petiole cortex. In one case this is in a fragment of what must have been a very large petiole, lying in the matrix remote from the main axis near the upper part of the plant. This fragment is very well preserved, though it is not repeated in the neighbouring two sections of the series, so that it gives no information as regards the method of branching. (See *Slide* T. 19.) The cortex is in part beautifully preserved, and lying in it a few cell rows from the outside to the left of the main stele is the small branch bundle. It consists of a rather scattered xylem group with some delicate soft tissue preserved, the phloem apparently lies almost entirely to the outside, and the whole is surrounded by a definite sheath of delicate small cells, most of which are not well preserved, but

some appear very clear and like endodermis. The second case is in the large petiole *ao* (see *Fig.* 1 and *Slide* T. 5) in the cortex of which lie two branches both far out. The neighbouring slides do not repeat them, so that little is gained, beyond the fact that when the petioles get some distance from the axis they do branch. Petiole *ao* (*Fig.* 1) is the largest and furthest out of the series.

The petioles give off a number of adventitious roots, though none shew the stelar branching with any degree of clearness. Among these numerous roots several small branches similar to them but of rather different type, are to be seen. These have rather more massive wood than the ordinary root, and have three or four protoxylem groups instead of the typical diarch stele. They may be small ramifications of the petioles, but there is no direct evidence that they are so. One only shews a well marked crescent stele (*Slide* T. 2), and this lies near a big petiole of which it may be a ramification, they are in the proportion of 2 mm. to 28 mm. in diameter, and their steles are orientated back to back, but there is no direct proof that the small one is a branch of this petiole in particular; though it is evidence that some of them must branch. Among the two or three small branchlets about the size of roots, but of uncertain nature, are two in which the steles have divided, each into a smaller and a larger portion (see *Slide* T. 25) but the series does not reveal what happens to the branches. We have, therefore, no clear case of branching among the petioles; a point in which this plant differs from the description of *T. solenites*, where, according to Stenzel the petioles branched repeatedly giving off small pinnules while they were still exceedingly near the main axis. He remarks on this as being unusual (p. 45), in the words that "ungestielte, gefiederte Blätter möchten bei lebenden Farnen kaum

vorkommen." Petioles so large as those of *T. Sutcliffii* would be likely to remain unbranched for some distance, and there is sufficient indication that in those further removed from the axis branching took place, to conclude that ultimately they may have ramified freely.

5. The Petioles.

The separation of the petioles from the main axis has just been described, as have the slight changes in shape undergone by the meristele during its passage out from the main stele. The individual petioles are approximately circular or oval in outline, with apparently no expanded portion or side wing at the base of attachment; their rather irregular outline at times appears to be the result of fossilisation. In some cases both stele and cortex are wanting, and there is only a large clear calcite mass about the shape and size of the petiole to represent it.

The **meristele** of a medium-sized normal petiole in most cases shews only the xylem in a good condition; between it and the preserved cortex is generally a gap larger on one side than the other, which probably represents the phloem, but is also partly due to the tearing away of the stele from the cortex, which is so common in ferns. In transverse sections the *protoxylem* groups stand out conspicuously, and are chiefly on the outer edge of the wood and on its convex side (see *Fig.* 8). In longitudinal sections, although many tracheides are of the simple scalariform type, others are pitted like those described for the main axis (see p. 51). In longitudinal sections a few cells are suggestive of spiral protoxylems, though it is doubtful whether they are so in reality; there are also a few cells which look like sieve tubes, but no trace of actual sieve plates is to be found. Several sections shew a zone composed of 3 or 4 rows of small thin-walled

cells just outside the xylem; these are probably the *phloem*, and they appear to surround the xylem on both sides. In some places, between them and the cortex is a darker streak of crushed cells, which probably represent the *endodermis* and *pericycle* in their normal position.

Several of the small branchlets, the nature of which is uncertain but which are possibly fine ramifications of the rachis (see p. 10), shew an extremely simple stele, the *xylem*, which alone is preserved, being a single solid mass, sometimes with 3 groups of protoxylem on the same side, like a very simple case of the form once called *R. tridentata*. In its simplest terms, this type of stele appears in the small branches of several ferns, and seems therefore to be of little value as a criterion of specific affinity.

In the small petioles (*i.e.* those which have recently left the main axis) the cortex appears to be a simple tissue of compact, probably rather thickened cells. In the larger ones, however, there seems to be a distinct differentiation into two zones, an inner one composed of thin-walled tissue which in tangential section has almost the appearance of "spongy parenchyma," and an outer one of rather thick-walled cells (*Pl.* 2, *Fig.* 2). Within the soft cells of the inner zone in many of the petioles are to be seen numbers of small circular or oval bodies, which have the appearance of actual cell contents (see *Pl.* 3, *Fig.* 9). In some parts they are exceedingly crowded, and bear a strong superficial resemblance to starch grains. Their likeness to fungal spores, as preserved so frequently in fossils, is also marked, but I incline to the view that they are starch rather than fungi, on the following grounds: Each little separate body is apparently unconnected with its neighbours by any thread or filament, as it would be if it were a fungal sporangium; there are further no obvious fungal hyphae which one would expect in con-

siderable numbers were the enormous masses of these grains of fungal nature; the tissues in which they are best found are well preserved, and shew no sign of destruction as might have been expected from masses of invading fungi; the grains are only present in the soft tissue, and not in sclerenchyma and stele also, as one might have expected had they been fungi; their size, shape, and general structure are exceedingly like recent storage starch in ferns; they are more numerous and conspicuous just in the positions one would expect were they starch in the rhizome and petioles of a recent fern, and appear in great numbers toward the apical part. Further, they are too deep seated to be chlorophyll grains, and also other indications tell us that the specimen was the underground portion of the plant. Prof. Oliver has observed somewhat similar structures in some new sections of *R. corrugata* Will., which are still undescribed. Williamson ('88) figured very similar bodies in the cortices of several plants, but inclined to the belief that they were algae, or intrusive unicellular organisms of some sort.

Some of the few minute branches which may possibly be the axes of pinnules (see p. 10) have apparently three zones of tissue in the cortex. Outside the space surrounding the stele is a zone of two or three rows of slightly thickened cells; outside them some soft tissue which is rather broken down, and which appears thin-walled, but varies in the specimens, and may only be a slightly less thickened zone continuous with the outermost zone of thickened cells.

The *epidermis* is seldom well preserved, and when present consists of small regular cells, in no way special.

As in the case of the main axis there appears to be no characteristic external point which might lead to the connection of this plant with other fragments.

6. THE ROOTS.

The many adventitious roots in the specimen originate from both the main axis and the petiole bases, in particular there is one or more (*Fig.* 4, *d*) coming off with each petiole stele as it leaves the main axis. Large numbers of the roots cut in several directions are present in each transverse section. They surround the axis and petioles, but each one is entirely free; there is no inclusion of the roots in the stem cortex, as is so common in tree ferns and the Psaroneae. They are of varying size, the larger ones being about 2 to 1·5 mm. in total diameter.

In some cases it is possible to see the dichotomous branching in transverse section, when the stele has equally divided and the cortex not yet completely separated.

The **stele** of an undividing root is of the simple diarch type so characteristic of ferns, and is composed of about 10 metaxylem tracheides, with a group of very small protoxylem elements at either end (see *Fig.* 6). These protoxylem groups are well marked in transverse section, but true spiral or annular elements have not been seen in longitudinal view. The larger tracheides are clearly pitted with multiseriate pits (see *Fig.* 7). As a rule there is no trace of the soft stelar elements, a space between the stele and cortex indicating their position.

The **cortex** is apparently uniform, composed of closely packed, rather thick-walled cells, which leave no air spaces. The outer layer does not seem to differ from the others, and neither root-hairs, nor exodermis has been found in any of the sections examined.

The roots as a whole shew no indications of having been specialised as air or water roots, and seem to be identical with the ordinary earth root of the modern fern rhizome.

7. Foliage.

No trace of foliage can be identified in any of the preparations. As the tissues preserved are those near the main axis, and the size of the petioles suggests a large leaf, there is a strong probability that no laminae would have been developed so close to the axis.

That the foliage was fern-like in appearance, the general anatomy can leave but little doubt. As was pointed out by Stenzel ('89, p. 4), in his description of the allied species, petioles of such great size indicate that the fronds must have been big, and were probably complex. It is very possible that some of the large frond impressions known generally as "tree fern" leaves, may belong to these plants. In *T. Sutcliffii* but two of the numerous petioles are seen to be branching to give off a pinnule rachis, but this in no way militates against the view that they were ultimately much divided.

Unfortunately, as before mentioned, p. 6, there is no characteristic feature on the cortex of either stem or petiole that might indicate which of the many unassociated impressions belong to this plant; hence it is unlikely that the connection of foliage impressions and structural material will be established in the way in which *Sphenopteris* and *R. aspera* were recognised as being the same. The probability is that until a happy chance reveals the secret in other structure-specimens, the foliage will remain unknown.

The likeness in general structure between the small petioles of *Anachoropteris rotundata* (*R. gleiche* Will.) and *T. Sutcliffii* makes it possible that some of the smaller ones known to us from our coal measures as *A. rotundata* may belong to *Tubicaulis*.

Petioles of *A. rotundata* of small size are now frequently

found in the lower coal measures, and any day they may turn up with foliage attached. It must not be assumed, however, that the plant has any connection with *Tubicaulis Sutcliffii* beyond the similarity in type of its meristele. As was pointed out long since by Williamson ('74, p. 676), similarity in structure of petioles alone is a very poor indication of true affinity, and is sometimes exceedingly misleading.

8. FRUCTIFICATION.

Unfortunately there is no fructification in organic connection with the plant. There are, however, several isolated sporangia scattered among the petioles and roots. The mere association of sporangia in such a position would carry but little conviction with it in the case of an ordinary bullion, which is packed with fragments of many things in extreme confusion; but in the sixty sections which this nodule has yielded, no plant structures are preserved but the one under discussion, a single minute fragment of cortex which looks as though it might belong to another plant, and these sporangia. It is generally the case that the "roof nodules" contain but one specimen (see p. 18), and, as a consequence, the importance of the association of the sporangia with *Tubicaulis* is considerable. Though there must still remain some doubt about the question, the probability is that they are indeed the fructification of *Tubicaulis Sutcliffii*.

Each sporangium is small, being from 0·2-0·15 mm. in diameter, and most of them are circular in outline. As is to be expected from their small size, each appears but once in the series of sections, so that it is impossible to determine whether the well-marked annulus is multiseriate or not. As is shewn in *Figs.* 10, 11, and 12, the annulus is well defined, and only on one side of the

sporangium, in which character they agree with *Botryopteris* rather than *Zygopteris*; the annulus appears to be of but one row of cells, but from the specimen it is impossible to be sure. Only the outer cells of the sporangium wall are sufficiently well preserved for recognition, though the débris on their inner sides indicates the possibility that they had the soft inner lining which occurs so often in fossil ferns No spores are preserved. One of the sporangia is bigger than the others and somewhat crushed, this shows the annulus very well (see *Fig.* 12), and may be more mature than the others. In one slide (T. 15) several sporangia are lying close together, but they are entirely free from one another. It is unlikely that sporangia of this type formed anything approaching a synangial sorus. These sporangia are extremely like those of a modern leptosporangiate fern, and there appears no reason to suspect them of being male fructifications, or in any way different from those of a typical fern.

9. Geological Horizon and Preservation.

The mine in which the nodule containing *Tubicaulis Sutcliffii* was found, is one which works the lower coal measure seam known variously as the "Bullion," "Upper Foot," or "Halifax Hard Bed," and is identified in many localities in Lancashire. The actual nodule was not one of the plant nodules found in the seam of coal itself, but was one of those in the "roof" which usually contain no plant remains, but are packed with goniatites of all sizes, many visibly sticking on the outside, and must undoubtedly have been formed under marine conditions. Sometimes such nodules contain plant remains, in which case there is generally but one plant in the nodule embedded among the goniatites and not mixed with

other plant débris. Each probably represents a drifted plant, which had become water-logged and sank among the shells on the sea bottom. This minimises chance association among plants so preserved in one nodule, for the vissicitudes of drifting would tend to separate any parts which were not organically connected. Hence the association of sporangia with the vegetative organs in the present instance is of much more value than it would be in the case of most fossils which have been preserved together with the mass of débris of the forest floor. In sections, the matrix of the nodule contains numbers of

Fig. 2. Small portion of the central axis shewing the "shattering" of the tracheides. (*Slide* T. 4.)

goniatites of varying size, some extremely small and only revealed by the microscope; they are embedded in what appears to be a fine mud which is nearly opaque in section, but which does not penetrate the tissues of the plant. Where, as is frequently the case, parts of petiole or axis are wanting, they are replaced by a clear, cream-coloured calcite frequently shewing crystalline structure. The preservation of the plant tissues is in parts excellent, but these good regions are tryingly local, and neighbouring sections may show a mass of calcite in place of the continuation of the tissues. Local "carbonisation" also occurs, which, though generally blurring the tissues, has

in this case preserved the phloem which is not otherwise recognisable (see p. 5, *Fig.* 5). Many of the tracheides in the best preserved portions show curious "shattering," the cause of which is difficult to surmise, and which is more marked in this case than in any other I have observed. The tracheid walls are not crushed, but have broken up into fragments as though they had been very brittle. (*Text-fig.* 2.)

The roof nodules, lying as they do immediately *above* the coal, must be slightly more recent in point of actual time than those bullions in the seam itself, though this difference is geologically so minute as to be of no moment. A more important difference than that of time seems to be indicated between the plants forming the "roof" and "seam" nodules, and that is the oecological conditions under which they grew. There are indications that in the "roof" nodules we are not dealing with a swamp flora, but one which had inhabited higher ground, so that it is likely that *Tubicaulis Sutcliffii* grew on dry land.

10. Affinities.

Among fossil plants the only one which shews a close likeness to this new fern is *Tubicaulis Solenites*, a plant which has been but once found, and that in 1815, near Chemnitz. It received its present name from Cotta ('32), and more recently was fully described by Stenzel ('89). It is not of Coal Measure age, being attributed to the "Rothliegende" (a division of the Middle Permian), but the anatomical details of axis, petioles, and roots are so strikingly alike in the two plants that generic identity is certain.

Owing to the kindness of the authorities of the Museum fur Naturkunde, Berlin, I have been able to handle parts of the original specimen of this unique plant,

while to Count Solms-Laubach I am indebted for the gratification of seeing a microscopic section shewing several of its petioles. From the examination of these specimens, as well as from the facts stated in Stenzel's description, I gather that though they are so alike, there is enough difference between them to indicate that the plants are specifically distinct.

As has been already stated, the preservation of *Tubicaulis Sutcliffii* is not propitious, and that of *T. Solenites* is also rather unsatisfactory, but in a totally different way, so that minute comparison of the two plants is very difficult. The tissues agree in a general way, and in the shape of the meristele and axis, and the size of the tracheides, the agreement is exact. *T. Solenites* evidently had pitted tracheides like those of *T. Sutcliffii*, though I did not see them in the sections. Stenzel describes them (p. 7) as having about four rows of short cross-pits, though he had no very satisfactory longitudinal sections to deal with. The differences between the two plants, which taken together appear to warrant specific separation, are the following:—The relative differences in the sizes of petioles and axes, *T. Solenites* with a main stele 6 mm. in diameter, has smaller petioles than *T. Sutcliffii* with a main stele 2 mm. in diameter. The petioles of *T. Solenites* seem to have branched much earlier than those of *T. Sutcliffii* (see Stenzel, *pl.* 1, *figs.* 2—8), giving off many pinnules very close to the main axis; this was pointed out by Stenzel as a peculiarity of this fossil plant, and unlike recent ferns. The phyllotaxy in *T. Solenites* is stated to be $\frac{1}{3}\frac{3}{4}$ and $\frac{1}{2}\frac{3}{7}$, while that of *T. Sutcliffii* is $\frac{2}{5}$. Further there is a considerable difference of horizon and locality, which presupposes a difference of species, as but few organisms run absolutely unchanged from the Lower Coal Measures to the Middle Permian.

The new species appears to be closely allied to the original species of the genus, and according to Stenzel's description of this limited genus, it is the only other known plant belonging to it. The name *Tubicaulis* has been given by a number of authors to different plants in the course of time, but none of these appears to have real connection with it, and were all discarded by him in his revision of the group in 1889. Since then Scott (:00, p. 296) suggested that Williamson's *Rachiopteris corrugata* would have been included in the genus by Stenzel, but now that more is known of the plant it is clearly seen to belong to another group.

The genus *Tubicaulis* is, therefore, a very small one, consisting of but two species, of each of which but one specimen is known. Stenzel placed it next *Asterochlaena*, to which it undoubtedly shows some affinity, but in Stenzel's monograph all the known forms of this series are not included, and it appears to be more like Renault's *Grammatopteris Rigolloti*. In Renault's description of this species '91, p. 16 and *plate x*, *figs.* 11 and 12) there appear to be many points in which it is like *Tubicaulis*, particularly in its simple solid circular stele, and in the simple band-shaped petiole steles, which, however, remain almost straight and do not curve to form a C as in *Tubicaulis*. *Botryopteris ramosa* (*R. ramosa* of Williamson '91 has also a similar solid main axis with simple masses of wood coming off to the petioles, but there any close agreement ceases; the arrangement and relative size of its petioles being very different from those of *Tubicaulis*. *Asterochlaena* in spite of its corrugated and irregular main stele, approaches more nearly to the type of *Tubicaulis*, and in the genus there are some varieties which may form a series. In *A. dubia* the simple C of the meristele bends its convex side towards the main axis as a *Tubicaulis*, in

A. krigisica the meristele is apparently unbent, while in *A. laxa* and *A. ramosa* the concave side is toward the main axis in the normal manner of ferns. In all cases the main axis is surrounded by a mass of petioles which come off in rapid succession, enlarging somewhat as they get further from it, but they never grow to so great a size as to exceed the main axis, a character which seems to belong to *Tubicaulis* alone.

Another group which may also be near these plants is the *Anachoropteris* series, in which the meristele is a single horseshoe shaped curve. Unfortunately the main axis is only known in one species, *A. Decaisnii;* this was described by Renault ('69) as being very like *Zygopteris*, that is, with a single five-rayed stele with the central vascular elements of smaller size than the rest and mixed with parenchyma. The meristele first comes off from this in the form of an elipse of hollow wood (see Renault, *plate* 10, *fig.* 4), which soon becomes an open curve. *A. rotundata* and *A. pulchra* have similar meristeles, which though detached are described by Corda ('67, p. 86-7) as being disposed with the concave side away from the main axis; this he gathered from the shape of the petiole cortex which seems to indicate the fact. If this is correct, then *Anachoropteris* is like *Tubicaulis* in this unusual feature as well as in the form of its meristele, though the latter is slightly more incurved and complicated in *Anachoropteris* than in *Tubicaulis* where it is absolutely simple.

All these plants belong to the large family of Botryopterideae, and within this group, somewhere near the base of the series among its simpler members, it will be safe to place *Tubicaulis*, near to *Grammatopteris* and *Asterochlaena*. The sporangia associated with *Tubicaulis* in no way militate against this view. To go into further

detail, however, and attempt a more ambitious classification, would be extremely unwise in view of the fact that next to nothing is known of the foliage and fructification of most members of the family.

A plant was described in 1849 as a "Tubicaulis von Ilia," and this Stenzel considers to be an *Osmundites*, a view which appears to be fully justified. He goes further, however, and considers that "im Sinne Cotta's" it should be associated with the *Tubicaulis* group, and he therefore places *Osmundites* after *Bathypteris* in the series, including *Zygopteris* and many other Botryopterideae. The detail of its woody structure, however, which does not agree with that of the Botryopterideae, as well as the enormous difference in time between the palæozoic group and the tertiary *Osmundites*, incline one to leave the two series for the present less closely associated than they are in Stenzel's table.

When we turn to recent ferns, there appears to be none which shows any close affinity with *Tubicaulis*. It is possible that through *Zygopteris* one may trace a family connection with the Hymenophyllaceae, but there is no direct likeness between them beyond a certain simplicity in the anatomy of the main stele, and it is doubtful whether that carries much weight. The small annulate sporangia associated with the fossil suggest a *Leptosporangiate* form, but without the foliage and sori one is at a disadvantage in discussing affinity.

Tracheides with multiseriate pits such as have been noted in *Tubicaulis* are found in most members of the group of the Botryopterideae, and they may well be of more interest and value than has hitherto been allowed. So far as I am aware they are not found among the living ferns, in which the scalariform elements are so essentially characteristic. On the other hand, well marked pits with

circular borders are equally a gymnospermic feature. Though we may find here and there instances to the contrary, taken as a whole there are few things so characteristic of the asexual pteridophyte and the asexual gymnosperm respectively, as the markings on their wood elements. Were the distinction between these elements with multiseriate pits and those with scalariform marking so unimportant as many suppose, then one would expect to find the former type among the ferns of to-day, but this, so far as I can ascertain, is not the case. Now the Botryopterideae, though they have scalariform elements, have many elements with series of pits recalling a *Lygino-dendron* and other primitive gymnosperms, and it appears that in this, and perhaps in this alone, they are not absolutety true ferns, but have taken one step towards gymno-sperm-anatomy. Other considerations lead one to suppose that they may be the group from which the Pteridosperms sprang, and it is not surprising that while perhaps remaining true ferns in every other sense of the word, some of them have assumed one feature in advance of the general run of ferns of to-day. As one of the openers of the discussion at the Linnean Society on the "Origin of Gymnosperms" this March (1906), Mr. Arber stated that there was every reason to believe that true ferns did exist in the *Palæozoic*, and that these would belong to a class from which both Leptosporangiate and Eusporangiate groups arose. To this old stock he gave the name "Primofilices," and stated that the Botryopterideae constitute one of its families, adding that the origin of the modern ferns from the Primofilices is clear. The point at present under consideration is whether the Botryopterideae are really members of the group Primofilices in the true sense of the word, which presupposes that its members shall be at least as primitive as those living to-day. Many of the Botry-

opterideae, however, appear to shew in the structure of their wood a character which places them *above* the modern fern, and, unless we are to assume that the modern ferns have lost the habit of forming such wood, and confined themselves to the simple primary wood of scalariform elements, then we cannot consider the Botryopterideae as a whole as Primofilicinean, though, of course, some of its members may have remained less advanced. The group of Botryopterideae appears to contain members which are "Pro-Pteridosperms" rather than Primofilices pure and simple, and with them was *Tubicaulis*, which I therefore conclude has no direct phylogenetic connection with any living fern.

There is one interesting group of "ferns" to-day with which they may have some remote affinity, and that is the Ophioglossaceae. This small family is by some considered to be the remnant of a group from which Lycopods and ferns sprang, but it has also certain anatomical characters which are perhaps recently acquired. In *Helminthostachys* bordered slightly oval pits are described by Farmer and Freeman (99, *figs.* 19 and 20 and in some material kindly supplied by Prof. Oliver, I have seen much more strikingly gymnospermic pits with perfectly circular borders. No direct connection with *Tubicaulis* is suggested, but it is possible that this group is in some points a nearer parallel to the Botryopterideae than are most of the recent ferns.

The fact that of the genus there are but two species, and that each has been found but once, with an interval of nearly 100 years, possibly indicates that *Tubicaulis* was one of the less successful types, which may very well have left no direct descendant among more modern plants.

II. SPECIFIC DESCRIPTION.

The genus as described by STENZEL ('89) is as follows:—

"**Tubicaulis** Cotta, Dendrol. S. 15.

"Truncus petiolorum basibus persistentibus obtectus herbaceus, fasciculo vasculari centrali simplice, terete, singulos fasciculos per corticem crassum in folia emittente. Petioli basi tenues, ascendentes valde incrassati, fasciculum vascularem simplicem, fasciaeformem, canaliculatum, cavitate extrorsum spectante includentes.

"Röhrenstein Breith.—Endogenites Sprengel p.p.—Selenochlaena Corda."

As before stated (p. 21), he included but one species in this genus, the description of which follows.

"Tubicaulis **Solenites**, Cotta.

"T. caule digitum crasso erecto herbaceo; cortice crassiore quam fasciculo vasculari simplice terete; petiolis basi tenuioribus, ascendentibus valde incrassatis, compressis, ultra pollicem latis, fasciculo centrali canaliculato, cavitate extrorsum spectante, cortice interno molli, externo e cellulis minoribus firmioribus composito."

To this I would add: Petiolis ramosis juxta caulem; phyllotaxis $\frac{5}{13}$ et $\frac{8}{21}$.

"Röhrenstein, Breithaupt in Oken's Isis, Jahrg. I. (1820), Heft. V., S. 440, Taf. 4. "Ueber eine Art von Palmen versteinerung: Röhrenstein."—Schippan, H. A., Quer- und Längendurchschnittsriss einer in Sachsen gefundenen versteinerten Palme. Freyberg, 1824. 4°. 6 Seiten und 1 Tafel.

"Endogenites Solenites p.p. Sprengel, comentatio de Psarolithis, 1828, p. 32.

"Tubicaulis Solenites Cotta, Dendrolithen. 1832, S. 21, 22, Taf. 2, Fig. 1—3.

"Selenochlaena Reichii Corda, Beitr. z. Flora d. Vorwelt. 1845, S. 81.—Unger, gen. et spec. pl. foss. 1850, p. 200—Goppert, Flora d. perm. Form. 1865, S. 44.—Schimper, traité d. Paléont. I. 1869, p. 697."

Tubicaulis **Sutcliffii** sp. nov.

Similis *T. Soleniti*; caule quam *Solenitis* minore, petiolis majoribus; phyllotaxis $\frac{1}{3}$; petiolis juxta callem sine ramis.

12. Summary.

Tubicaulis Sutcliffii is a new species discovered at Shore in the roof of the Bullion seam, Lower Coal Measures.

The single specimen consists of an *axis* with a solid monostele, surrounded by numerous petioles, each with a simple C-shaped meristele with the *convex* side toward the axis.

The petioles are small on leaving the axis, but rapidly increase till they greatly exceed the axis in diameter.

The phyllotaxy is $\frac{1}{3}$; no "axillary shoot" has been observed.

Many adventitious roots arise from petioles and axis, which branch freely and generally dichotomise.

No foliage is associated in any way, though compound leaves are suggested.

Several small annulate sporangia are associated, and as the nodule is a 'roof" one this strongly indicates true connection.

The plant is closely allied to *T. Solenites* Corda, but is specifically distinct.

It appears to be one of the simpler Botryopterideae, and to have no direct affinity with any living fern.

In conclusion I must acknowledge my indebtedness to several people in the course of this work. To W. H. Sutcliffe, Esq., as before stated, for the generous way in which he placed the specimen in my hands; to the Authorities of the Berlin Museum and Count Solms Laubach for the loan of their valuable type specimens of the allied species *T. Solenites*; to Prof. Weiss, in whose department the work was done, for his kindly and helpful interest; to Prof. Oliver, for permission to examine the specimens of Botryopterideae at University College, and other kindness; and to Mr. D. M. Watson for photographing a number of the sections. I may add that owing to Mr. Sutcliffe's generosity, the whole of the specimens have been deposited in the Manchester Museum, with the following exceptions:—Sections T 13, T 4, L 14, have been presented to Prof. F. W. Oliver, section T 16 to Mr. James Lomax, sections T 9 and L 12 have been retained by Mr. Sutcliffe, and T 12, T 22, T 26, and LE 2 by myself.

13. LITERATURE.

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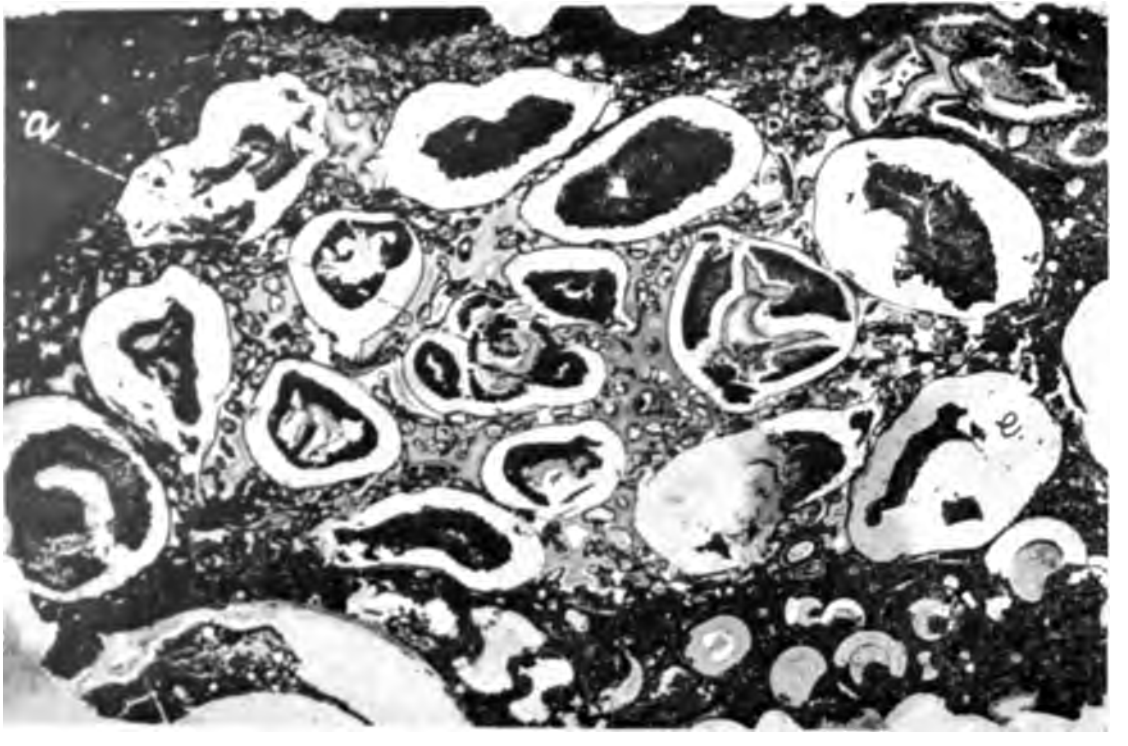
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Plate 2.

Fig. 2. Several roots cut in different directions, root *a* magnified in *Fig.* 6. *b* part of outer cortex of a petiole.

Fig. 3. Central axis, showing branches of stele going to petioles and roots. × 14.

a. Solid central meristele; its star shape is due to the local breakdown of Tracheides.

b. Strand just leaving for a petiole.

c. Meristele now enclosed in petiole cortex.

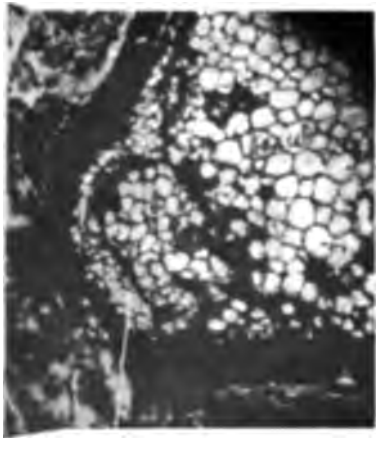
d. Root bundle coming out at the side of the petiole.

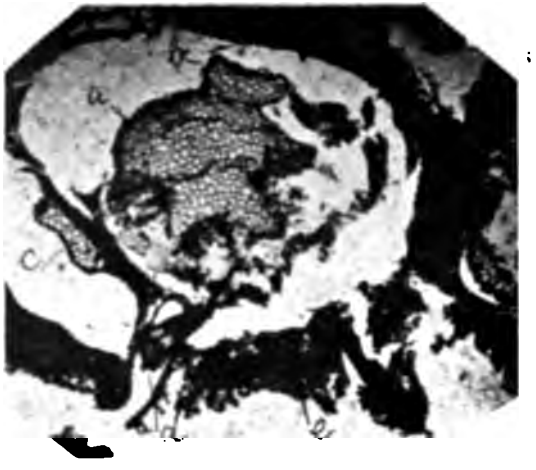
e. Cortex of main axis.

Fig. 4. Similar to *Fig.* 3. The root *d* cut in transverse section.

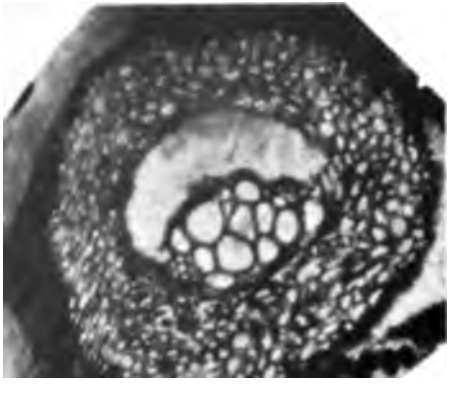
Fig. 5. Small portion of the main axis, shewing a strand *b* beginning to separate from the main mass of the wood *w*. Surrounding the wood and within the dark band lie the smaller, thin walled phloem cells, *ph*. (*Slide* Z 1.)

Fig. 6. Root *a* of *Fig.* 2 enlarged. Shews the diarch stele with large metaxylem elements (*cf. Fig.* 7).




a
b
c
d
e

a
b



a
b
c

Plate 3.

Fig. 7. Longitudinal section of a root shewing the multiseriate pits of the woody elements. × 140. (*Slide* L 6.)

Fig. 8. Small portion of a petiole shewing a piece of the stele and the cortex. × 44.

w. Wood with the protoxylems, *px.*, on the convex side.

s. Crushed cells, probably phloem and sheath.

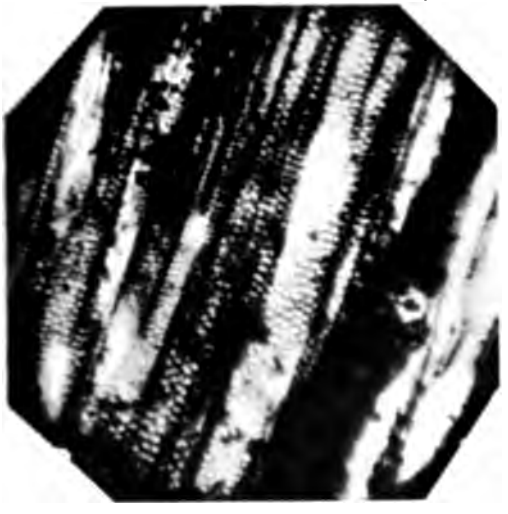
c. Cortex.

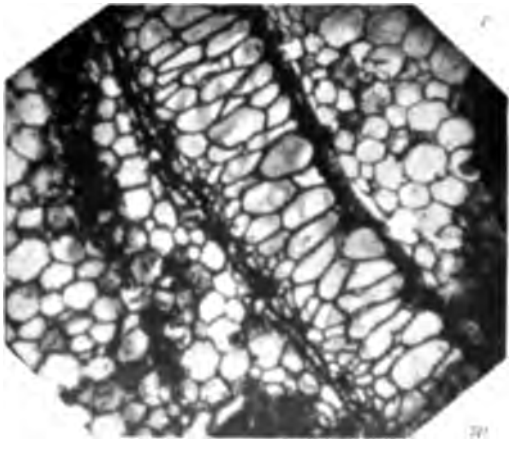
Fig. 9. Drawing of cells from the cortex containing circular bodies, possibly starch. × 400.

Fig. 10. Photograph of a sporangium. × 184.

Fig. 11. Drawing of a sporangium shewing the circular form and well marked annulus. × 230. (*Slide* L 1.)

Fig. 12. Drawing of cells from the annulus of a larger sporangium. × 290. (*Slide* L 9.)





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XI. On the Difference between Physiological and Statistical Laws of Heredity.

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Contents.

1. Introductory. p. 1.
2. Statistical Laws.
   (*a*) Pearson's Law. p. 4.
   (*b*) Galton's Law. p. 4.
   (*c*) The difference between the two. p. 5.
3. Physiological Laws.
   (*a*) The Law of Diminishing Individual Contribution. p. 8.
   (*b*) Mendel's Law. p. 11.
   (*c*) The difference between the two. p. 14.
4. The Difference between statistical and physiological Laws.
   (*a*) The former "descriptive": the latter "explanatory." p. 15.
   (*b*) Mendel's Law true of units: Pearson's of masses. p. 19.
   (*c*) Examples of the confusion between physiological and statistical Laws. p. 23.
   (*d*) Description of a method of dealing with the material of a breeding experiment in such a way that the data obtained may be used to test the validity both of Mendel's and of Pearson's Law. p. 28.
   (*e*) Why do white sheep eat more than black ones? p. 31.

July 6th, 1906.

I.

There are those who maintain that it is not the part of the biologist to argue, to discuss, and to explain; and who assert that he is transgressing his proper limits when he ceases to confine himself to the description of observations and experiments, and to drawing from them certain obvious conclusions.

I do not hold this opinion: because I am convinced that if as much (I do not say more) trouble were taken to understand the meaning of a term as is spent in establishing the authenticity of a fact, the progress of our knowledge of fundamental natural processes—heredity, variation, the determination of sex, to name a few—would be more rapid than it is at present. For it seems to me to be evident that nothing short of a firm but unprejudiced grasp, in the mind of the investigator, of the relation between the facts themselves and past, present and possible attempts to account for them can enable him to advance toward a closer knowledge of these phenomena.

I think that the reader will admit the truth of this contention, if he is not one of those who still cling to the Baconian delusion that all that is necessary for the elucidation of the problems of nature is the bringing to light of as many facts as possible by as many workers as possible; whereas as a matter of fact it is obvious that that which hinders the progress of natural knowledge is not the slowness with which facts are brought to light, but the paucity of investigators capable of dealing with them properly.

The incapacitating fault among biologists which is at once the commonest and the most serious is the unconscious ease with which they fall into the error of using a term without having previously ascertained its meaning. And so long as biologists turn a deaf ear to speculation

this disease will flourish. That which is necessary, therefore, to make progress both surer and swifter is a greater aptitude to formulate a clear idea of the meaning of the terms which are employed—a habit of mind which is not likely to be common so long as the consensus of biological opinion regards with less favour the attempt to discover the essence of a newly suggested hypothesis, than the attempt to describe the course of the vas deferens in a newly discovered worm.

In the study of heredity in particular the most extraordinary confusion has resulted from the fact that not only has the same term been used to mean different things by different writers, but very often has had many significations in the writings of a single author. This state of affairs is due, in my opinion, to the absence of any patient and laborious attempt to thresh out the meanings of the terms continually on the lips of those who take part in the discussion of this subject; and this absence is due in its turn to the callousness, if not disfavour, with which such an attempt is likely to be regarded. Nevertheless I propose to make it.

Space forbids me to discuss the question of the advisableness of using the term "law" *at all* as summarising vital phenomena, more than to say that the fact that I use it 166 times in this paper demands some apology.

I use it because, besides possessing the advantage of brevity, it is of all terms in biology the vaguest; signifying as occasion demands either a theory, or a résumé, or a hypothesis, or a formula, or a generalisation—to name a few of the more or less legitimate senses in which it is used: and because it shelters, under its wide roof, Laws whose authors aim at explanation, and those whose authors are satisfied with description. And I spell it with

a capital L because that is the conventional way of writing terms the discussion of whose meaning is postponed.

2 (*a*). *Pearson's Law.*

No one has any excuse for not knowing what the Law of Ancestral Inheritance is; the essential features of it are outlined by Pearson in the following words:—

"Taking our stand then on the observed fact that a knowledge neither of parents nor of the whole ancestry will enable us to predict with certainty in a variety of important cases the character of the individual offspring we ask: What is the correct method of dealing with the problem of heredity in such cases? The causes *A*, *B*, *C*, *D*, *E*, ... which we have as yet succeeded in isolating and defining are not always followed by the effect *X*, but by any one of the effects *U*, *V*, *W*, *X*, *Y*. We are, therefore, not dealing with causation but correlation, and there is therefore only one method of procedure possible; we must collect statistics of the frequency with which *U*, *V*, *W*, *X*, *Y*, *Z* respectively follow on *A*, *B*, *C*, *D*, *E*.... From these statistics we know the most *probable* result of the causes *A*, *B*, *C*, *D*, *E* and the frequency of each deviation from this most probable result. The recognition that in the existing state of our knowledge the true method of approaching the problem of heredity is from the statistical side, and that the most that we can hope at present to do is to give the *probable* character of the offspring of a given ancestry, is one of the great services of Francis Galton to biometry.*

2 (*b*). *Galton's Law.*

Galton formulated his Law as follows: "The two

* Pearson, 1903*a*, p. 215.

parents contribute between them on the average one-half, or (0·5) of the total heritage of the offspring; the four grandparents, one-quarter, or $(0·5)^2$; the eight great-grandparents, one-eighth, or $(0·5)^3$, and so on. Thus the sum of the ancestral contributions is expressed by the series $\{(0·5)+(0·5)^2+(0·5)^3$, &c.$\}$, which, being equal to 1, accounts for the whole heritage." *

2 (*c*). *The Difference between Pearson's and Galton's Law.*

It will be seen how profoundly Galton's differs from Pearson's Law. Yet the belief that the two are much the same is not rare, and the statement that the latter is merely an extension of the former is often made. A clear appreciation of the difference between the two is necessary to anyone who wishes to be conversant with modern theories of heredity.

One feature the two have in common; both of them are true only of masses, and do not pretend to apply to individuals. This is so obvious to the careful thinker that Pearson only refers to it in a footnote;† yet it is often forgotten. The difference between the two lies in this: Pearson's Law measures the degree of correlation between a character or characters in a given generation, and some similar (or dissimilar) character or characters in the preceding generation. Galton's Law states the amount which a given generation *contributes*‡ to the generation which it produces. It definitely states that on the average a half of the filial generation are like the parental, a quarter like the grandparental, and an eighth like the great-grandparental, and so on. From the knowledge that the parents of a given generation of cats are tabbies,

* Galton, '97, p. 402.
† Pearson, :04, p. 161.
‡ See Appendix B.

and that half of its grandparents are tabbies, a quarter whites, and a quarter blacks, you are enabled to predict by Galton's Law the proportions in which these three kinds of cats will occur in that generation. Pearson's Law does not enable you to do this: it is of an entirely different kind. For Pearson's Law to be true it is not necessary

FATHERS.

CHILDREN.	Black.	Dark Grey.	Grey.	Pale Grey.	White.	Totals.
Purple.	3	11	2			16
Bluish Purple.	8	27	19	8		62
Purplish Blue.	6	30	39	32	3	110
Blue.		12	27	33	18	90
Pale Blue.			4	10	8	22
Totals.	17	80	91	83	29	300

The degree of correlation in the above Table is that between the number of dice exhibiting 4 or more than four points (or "pips") uppermost, in first throws and the number exhibiting those faces uppermost in second throws in a series of 300 double throws. The two throws are correlated by leaving half the first throw on the table, so that the second throw has half the dice lying exactly as they fell in the first throw. For details of this process the reader is referred to Weldon :06, p. 100.

The degree of correlation in my imaginary Table is ·54; for calculating which I am indebted to Mr. Udny Yule. The "coefficient of parental heredity," therefore, in this case is identical with that for the inheritance of deafness, of which Pearson's Law is true, recently worked out by Schuster (:06, p. 478). Yet, in my imaginary case, none of the children could be mistaken for any of the parents.

that *any* of the children should be like *any* of the parents; all that is necessary is that a particular kind of parent should be associated with (*i.e.* should produce) as often as not a particular kind of child. On the opposite page is an imaginary Correlation Table, in which Pearson's Law is borne out, yet in which none of the children are like any of the parents.

The fact that the relation between a given generation and those that precede it, is described by a series of figures which in the case of Galton's Law is ·5, ·25, ·125, ·0625 etc., and which in the case of Pearson's, for eye colour in man, for example, is ·4947, ·3166, ·1879,* has led some to believe that the figures mean the same thing (which, of course, they do not), and has thus constituted a trap for the unwary. Castle has done good service to progress in the study of heredity by falling into it.†

I hope I have made clear what the difference between the meanings of the two series is; for to understand this is to understand the difference between the two Laws.

This difference is sometimes expressed in the statement that Pearson's Law is more comprehensive and less biological‡ than Galton's: and inasmuch as it embraces sets of facts which are not described by Galton's formula, the first of these statements is true: and inasmuch as the relation between successive generations which it measures is the same as the relation between two series of throws of dice (in which reproduction is unknown), of which every throw of the second series consists of half the dice lying exactly as they fell in the corresponding throw of the first series, the second is true also.

* Pearson, 1904, p. 221.

† Castle, '03, p. 224.

‡ Fruwirth (1905, p. 147) goes so far as to say that "das Ahnenerbengesetz ist kein biologisches Gesetz, ..."

3 (*a*). *The Law of Diminishing Individual Contribution.*

In my paper on the supposed antagonism of biometric to Mendelian theories of heredity, I showed that a set of facts (summarized in the Table on page 6 of that essay), appearing at first to be a complete refutation of Mendel's Law, could easily be shown to be equally in accord with both Mendelian and Galtonian theories.* Mendel's Law describes the individual phenomena in this case *perfectly*: Galton's Law describes the mass result composed of these very individuals mating at random *perfectly*. The latter describes the proportions, the former accounts for them. The Galtonian deals with individuals from the point of view from which the physicist deals with atoms; the Mendelian deals with them from that of Clerk Maxwell's demon.

Now just at the same time that I announced my discovery that the proportions of the albinos in this case were not evidence against the truth of Mendel's law,† Castle made the same discovery.‡ But he argued from his discovery, not (as I did) that the two theories were compatible but that Galton's was wrong; that is to say, he must have thought that the two theories were mutually exclusive; which indeed he did: but not in the same way that I did. For whilst the way in which I made that error was by lifting Mendel's Law from the level of a would-be explanatory to that of a purely descriptive Law, he made it by lowering Galton's from the level of a purely descriptive to that of a would-be explanatory one. And the reason that I discovered my mistake before he did was that it is easier to see that Mendel's Law is something more than a purely descriptive one, than it is to see that Galton's is not a would-be explanatory

* Darbishire, :05*a*, p. 6.

† Darbishire :05*a*, p. 9. ‡ Castle :05, p. 17 *et seq*

one. And the reason again of this is that Galton's Law is confused with another one which resembles it in one respect, but differs from it in being would-be explanatory. The remarkable thing about this Law is that whilst it is characteristic of most Laws to be enunciated and receive a name first and then become widely believed in afterwards, the reverse is the case with this one; for it is believed in by all biologists who are not Mendelians, by all breeders of animals or plants, and by all persons not belonging to these classes who think about heredity at all.

But it has not yet received a name. I propose to call it the *Law of Diminishing Individual Contribution.*

According to it: *the germ plasm of an individual contains contributions from all of its progenitors: the amount of the contribution being large in proportion as the progenitor is near, i.e.*, large in the case of the parents, smaller in the case of the grandparents, and so forth.

It is a very good type of biological Law: it has the advantage of simplicity: it is also, except in a few cases, untrue.

I will now give 3 cases to shew how widespread belief in this Law is.

The first that I give is that of the result of crossing a yellow and white pink-eyed Japanese waltzing mouse with a pink-eyed white mouse—that is, an albino. The result is, usually, a black-eyed grey mouse.* And to anyone not familiar with it, the result is most astounding: it is quite the opposite of what one would expect. Expect from what? From one's—possibly unconscious—belief in the Law of Diminishing Individual Contribution.

Another case. Now that we know that a blue Andalusian fowl is a heterozygous form produced by mating a black and a white; and that Andalusians when mated

* Darbishire, :04*a*, p. 7.

together produce 25 per cent. Blacks, 50 per cent. Andalusians, and 25 per cent. Whites, we no longer try to get a pure strain of Andalusians by throwing away the blacks and whites and by continuing to breed from the Andalusians for many generations, because we know that we can always get Andalusians and nothing else by mating blacks and whites*. What is the conception of heredity which underlay the old-fashioned attempt to breed pure Andalusians by weeding out the blacks and whites, but the Law of Diminishing Individual Contribution?

Again Coutagne†, in discussing the possibility of the hybrid nature of some dark-lipped individuals of *Helix hortensis*, which occurred in a collection of that species and *Helix nemoralis* living in one locality, concluded from the fact that these supposed hybrids were *unbanded*, whereas the great majority of the *H. nemoralis* in that locality were banded, that they were *not* hybrids. To translate his own words "If the *H. nemoralis* were the parents of the 113 black-lipped individuals there is every reason to believe (*tout porte à présumer*) that this character of banding would appear at least in some cases in these 113 individuals." Through Lang's‡ work we know now that in a cross between a banded *H. nemoralis* and an unbanded *H. hortensis* the unbandedness is dominant. So that now we should *not* expect "this character of banding" to appear in *any* of the individuals: and Coutagne's argument falls to the ground.

But what is "*tout porte à présumer*" but the expectation based on a firm belief in the Law of Diminishing Individual Contribution?

* Punnett, :05, p. 28.

† Coutagne, :95, p. 72.

‡ Lang :04 (p. 497) and :06. See also Darbishire :05*b*, p. 196.

These three cases shew how widespread is belief in this Law; and they also shew that in these three cases at least it is not valid.

The difference between the expectation based on this Law* and the accurate knowledge of what actually takes place (which it is the business of Mendelian investigation to supply), is the same as the difference between common sense and science, and the same as the difference between that which stands to reason and that which rests on evidence.

3 (*b*). *Mendel's Law.*

I do not propose to discuss here the statement that the time has not yet come when we are justified in speaking of Mendel's *Law*, nor to enquire into the meaning of this statement: the question I propose to answer is, "What is the essential feature of that which is called Mendelism by those who believe in it, and Mendelianism by those who do not?"

I divide definitions of the Law into two primary categories:—

(i.) Suitable for those who desire to establish the invalidity of the Law.

(ii.) Suitable for those who wish to discover whether Mendelism has helped us or is likely to help us to attain to a more intimate knowledge of heredity.

There is no difficulty in finding a definition of the first class: a very satisfactory one is one which binds the Mendelian down to the Law exactly as enunciated, and the description of the phenomena exactly as given by Mendel himself. If this is carefully done, no difficulty

* Huxley must have been thinking of some such Law as this when he made the remarkable statement that science was organised common sense

will be encountered in establishing the invalidity of Mendel's Law as facts accumulate.

To discover a definition of the second class is not so easy. To my mind, there are two perfectly distinct things included under the one term Mendelism. One is belief in the existence of character-units in the germ, and in the thesis that these units are pure in respect of the characters which they represent. The other is the *method* by which the extent, separateness, and transmission of these units is discovered. The first may be called the Mendelian theory, the second the Mendelian method—which is the application of the experimental method to the study of heredity. I think that both these things are implied when the term Mendelism is used; and whether they are or not—and it does not in the least matter—I believe that the Mendelian method will do as great service, in accounting for the phenomena of heredity, as that particular theory which Mendelians happen to be employing at the moment.

For I think that it must be evident to anyone who has followed closely the Mendelian work of the last few years that, while the method which workers of that school have employed has remained the same, the actual theories, by testing the validity of which they have sought to attain their end, have been from time to time considerably modified. This procedure, of course, makes it difficult for those who wish to criticise or base statistical calculations on the theory itself. Pearson comments on it in these words: "The original Mendelian theory has been replaced by what are termed "Mendelian Principles." In this aspect of investigation the fundamental principles propounded by Mendel are given up, and for each individual case a pure gamete formula of one kind or another is suggested as describing the facts. This formula is then emphasized,

modified, or discarded, according as it fits well, badly, or not at all with the growing mass of experimental data. It is quite clear that it is impossible while this process is going on to term anything whatever Mendelian as far as theory is concerned."*

But should we be right in refusing to commend the efforts of a well-digger, if, in sinking his well, he alternately used a spade, a pickaxe, and dynamite, according as he had to deal with gravel, sandstone, or granite, provided that he found, or even that he thought he would find, water at last?

The aims of the Mendelian and the well-sinker are the same—to discover something; and they each employ a definite method, but the tools they use are continually being changed. That is why I think that the method is at least as essential a part of Mendelism as the theory. And that is why I think that there is no more connection between Pearson's generalized theory of alternative inheritance (with special reference to Mendel's Laws), and Mendelism, than there is between the second law of thermo-dynamics and the Maxwellian demon's knowledge of atoms *plus* the method by which he has acquired it.

There *is* a definite relation between a generalized theory of alternative inheritance and that particular doctrine on which it is based: it is the same as the relation between the second law of thermo-dynamics and the theory held—ex hypothesi—by the demon as to the nature of the atom.

But there *can* be no relation between any generalized theory of inheritance and Mendelism† unless that term

* Pearson, '03', p. 53.

† I do not, of course, intend to imply that Pearson tries to establish any relation between a generalized theory of inheritance and Mendelism: I *know* his was a generalized theory of alternative inheritance based on the theory of the pure gamete. All I wish to insist on is that the theory which

signifies the Mendelian theory *only*; and, even so, this relation cannot be permanent unless the Mendelian is pledged not to change his theory in the smallest degree. I hold that, in the first place, Mendelism has, as I have shewn, a wider signification, namely, that it embraces the method as well; and secondly, that no Mendelian can be expected to take the pledge demanded, if, by doing, so he believed, as he probably would, that he would be prevented from attaining his end. It is idle to accuse him of inconsistency. What should we think of the consistency of a well-digger who died of thirst because he would stick to his spade although only a few feet of granite separated him from water?

And what right have we to expect that the demon should pledge himself not to alter his theory of the nature of atoms if he hopes that by being free to alter it he will attain a knowledge of them that will enable him to live in a warm compartment without having to do any work for it? What right has a physicist to expect a demon not to alter his theory, on the ground that such an alteration makes it exceedingly difficult for him (the physicist) to use the theory as a basis for statistical calculation?

3 (*c*). *The difference between Mendel's Law and the Law of Contribution.*

Now that we come to discuss the difference between Mendel's Law and the Law of Diminishing Individual Contribution we must clearly understand that by Mendel's Law we mean the theory associated with that name, and not the method.

Mendelians happen to be testing at the moment is, to my mind, not the essential thing in Mendelism. If the commonly accepted explanation of the proportion 1DD : 2DR : 1RR were shewn to be false, would experiments, called Mendelian, now in progress be prosecuted with less zeal? By no means. Such a discovery would even be an incentive to more strenuous search.

These two Laws resemble each in being physiological, in that they attempt to picture the way in which characters are represented in the germ cell. But they differ profoundly in the picture which they draw. The difference is so obvious that it is hardly necessary to speak about it. A remark on the difference between the predictions of the two Laws as to the nature of the offspring of extracted recessives will suffice. Suppose that two hybrid mice with grey coats and black eyes were to produce an (extracted) albino—which, if the Law of Contribution were true, they could not do: the Mendelian prediction about the offspring of a pair of such albinos is that they will be all albinos: the expectation based on the Law of Contribution is that a quarter of the coat of each individual child will be grey—supposing the proportions for individuals in which each progenitor contributes, according to that Law, to be the same as that demanded for populations by Galton's. The Mendelian prediction is right.* In fact, the Law of Contribution is so utterly invalid that every case of alternative inheritance is a contradiction of it. It may apply to some cases of blended inheritance. But the reason that I have formulated it, and given it a name, is not that it may perhaps apply to one or two cases; but because unless it is definitely enunciated it will not be reckoned as having any claims to recognition; and because, the sooner it is widely recognized, the easier will it be to put an end to its confusion with Galton's Law.

4 (*a*). *Statistical Laws, "descriptive": Physiological Laws, "explanatory."*

The remark might be made about the Law of Contribution that it is Galton's Law made applicable to the

* Darbishire *op. cit.*, p. 23.

individual: and this in a sense is true: but there is a profound difference between the two; for, whilst the Law of Contribution is an attempt to picture the way in which characters are represented in the germ cells of individuals, Galton's Law is merely a statement that the characters of the ancestry of a population reappear in certain definite proportions in that population. It is only concerned with that which is above the horizontal line *AB* in the figure on the next page. Moreover it is only true of the aggregate of adults, and not of the individuals which compose that aggregate. The Law of Contribution, on the other hand, deals with that which is below as well as with that which is above the horizontal line, and it is true of the individuals.

Now, although Galton's Law is true of the mass but not of the component individuals, the Law of Contribution is true of the individuals and of the mass as well: because according to it all the individuals are contributed to, in the same degree.

The widespread belief in the existence of a Law which is true both of mass and individual is the result of the interaction of two outstanding characters of the human mind, (i.) an inability to distinguish between truths about masses and truths about individuals—and (ii.) a passion for explaining things; for possessing a formula for familiar phenomena; in the case of heredity for seeing below the line in the diagram opposite.

Man has observed that on the whole Galton's Law is true of masses—though he has not expressed himself in these words. Feature No. (i.) had led him to think that the truth is of individuals, and has thus satisfied No. (ii.) and supplied him with a brief formula, a simple picture of the way in which characters are inherited—a picture of what is below the line *AB* in the diagram.

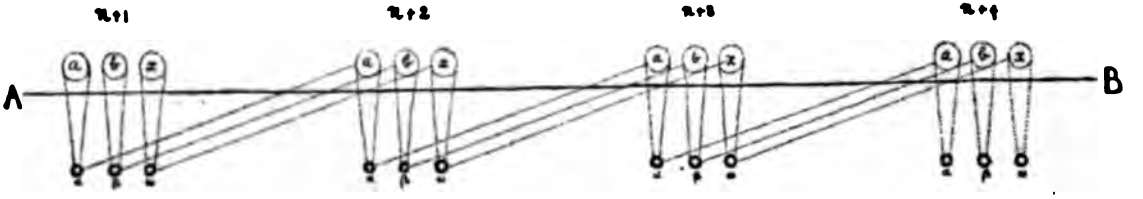

DIAGRAM TO ILLUSTRATE THE DIFFERENCE BETWEEN PHYSIOLOGICAL AND STATISTICAL LAWS OF HEREDITY.

$n+1$, $n+2$ ... etc. represent successive generations. a, b, x the adult individuals of a given generation; and α, β, ω the germ cells formed by those individuals.

(The fact that I have not joined the sets of germ cells (α, β, ω) by lines may seem to indicate disbelief on my part in Weismann's theory of the continuity of the germ-plasm. It does not.)

Even if this is not a true history of the origin of the Law of Contribution, the point about that Law which I wish to enforce is that it is a physiological theory of heredity. It is an attempt to account for the phenomena of heredity by picturing the way in which the characters of an organism are represented in the germ cell which produces it. And being a physiological Law, it is profoundly different from a statistical one like Galton's, which does not pretend to account for anything, but is a generalization about the relation between the aggregates of adults of successive generations.

In a word: a physiological Law deals with the individuals of each generation on both sides of the line *AB* in the diagram: a statistical one, with the generation as a whole on the upper side only. Another way of marking the difference between physiological and statistical Laws of heredity is to say that the former are explanatory while the latter are descriptive. To which it will be immediately objected that no Law ever explains anything. I am perfectly aware of this, and of the fact, moreover, that no theory does; in fact that we cannot *explain* anything at all in nature; that all that we can do is to describe. But at the same time it cannot be denied that there is all the difference between attempting to account for a phenomenon and contenting oneself with describing it: and one is, I hold, perfectly logical in making this attempt to explain, although one knows that however intimate a knowledge of causation one has acquired one has done no more than describe phenomena. One great difference between these two things, the logical attempt to explain, and satisfaction with mere description is that the method of the former is experiment and that of the latter is observation. Another difference is that it is only by attempting to account for things that we have been enabled to get what

knowledge we have of the causation of natural phenomena and so to obtain what control we have of the operation of natural processes. Just because we know that explanation is after all only description, it does not follow that we should abandon the attempt to account for things.

We are now in a position to classify Laws of heredity under these two headings.

Physiological.	*Statistical.*
(*a*) Mendel's Law.	(*a*) Galton's Law.
(*b*) The Law of Contribution.	(*b*) Pearson's Law.

Now inasmuch as of physiological Laws *b* has been shewn to be invalid, and of statistical ones *a* has been shewn to be less comprehensive than *b*, the discussion of the mutual relation of physiological to statistical Laws of heredity resolves itself into a discussion of the relation of Mendel's Law to Pearson's Law of Ancestral Inheritance.

4 (*b*) *Mendel's Law True of Units: Pearson's of Masses.*

Discussing a little while ago with a friend the relation of Mendelism to biometry, I suggested as the briefest possible statement of the difference between the two that Mendelism treated of units, and biometry of masses of units: my friend replied. 'You speak of the animals and plants with which you deal as breeding true to such and such a character. What do you mean by this statement? Are the offspring absolutely identical with their parents in respect of the character under consideration? If they are not, how like are they? and how is the degree of this similarity to be measured, except by biometric methods?'

I saw that there was truth in what he said; but I could not see the relation which the idea in his mind bore to my original idea, to which I still adhered,

that Mendelism dealt with the units while biometry was concerned with masses. Now I see that my inability to do so was due to the fact that biometry meant to my friend that the only way to measure the resemblance between parents and children is the method of the correlation table; in which he was quite right: while biometry called up in my mind the Law of Ancestral Inheritance, and especially the manner in which data designed to establish that Law are collected and dealt with; such, for example, as in the case of one of the last series of data from which such correlation coefficients have been worked out—that of greyhounds. I had in my mind the collective treatment in one correlation table of such different characters as Black, Brindled, Fawn, White, and Red;* while he was thinking of the only method of measuring the intensity of inheritance within a single such character—say, Red. Now this I believe brings us to the heart of the matter. When I say that the Mendelian deals with units and the biometrician with masses, I mean, not that the former deals with a few and the latter with many, but that the former first settles what character he is going to treat as a unit and then only deals with it in large numbers when he is sure that the component units of this number are identical; their sameness having reference to such properties as can be discovered by mating them with their like and with their unlike. It is just as necessary for the Mendelian to have a large record of matings, as the biometrician, to establish his generalizations. But though the Mendelian might allow that the only method of measuring the similarity between parents and offspring within a group (such as the unit character Red) was that of the correlation table, he would vehemently maintain that the biometric method over-

* Barrington, etc. :05, p. 264.

stepped its limits when it included in such a table more than one such category. What Bateson means when he says that Mendel saw by sure penetration that masses must be avoided is that the biometric method oversteps its limits when it does this.

The answer that I should now give to my friend is this: 'I fully admit that the only method of measuring the degree of resemblance between a generation and the one which produced it, within such a unit, is the biometric method: I fully agree with the biometrician, when he says that all green peas are not alike in respect of their greenness because all green peas are green, and that the biometric method is the only one to measure their dissimilarity; but I stoutly maintain that when he puts green and yellow peas together into a correlation table he has started on a path which will not lead to a more intimate knowledge of heredity.'

Biometry furnishes the only means of actually measuring the intensity of heredity within a unit: Mendelism furnishes the only means by which a fuller knowledge of the properties of these units may be acquired.*

It is sometimes easy to determine the extent of these units as in the case of discontinuous characters such as purpleness and whiteness of flower in *Pisum*: † it is often difficult as in the case of characters varying continuously such as the weight of beans.‡

Careful consideration of the table on p. 6 of my paper § on the supposed antagonism of Mendelian to biometric theories shews that the accuracy with which Mendelian

* Exactly the same idea is expressed by Lotsy (:06, p. 143).

† Frurwirth :06, p. 141.

‡ Johannsen :03.

§ Darbishire :05.

or Galtonian Law describes the facts of heredity depends on the composition of the unit tested. There is nothing *a priori* illogical in treating the sharply defined category of dark-eye-and-coloured-coat, in mice, as a unit. Suppose this is done: Galton's Law fits the facts beautifully, while Mendel's is triumphantly refuted (by shewing that the amount of albino ancestry of a hybrid affects the percentage of albinos produced by such hybrids mated *inter se*): to which the Mendelian would make the following answer: "Are you sure that your unit 'dark-eye and coloured-coat' is incapable of resolution into still simpler units. Are you sure that you are not regarding as a simple thing that which is really compound, just as the 'fixed alkalies' were regarded as elements until Davy showed them to be compounds.*† I can prove that you are: for by testing the gametic constitution of the dark-eyed and coloured-coated forms, I can shew that they are sharply distinguished into heterozygous and homozygous forms. Now I claim to have discovered what should really be treated as a unit; namely that character which, like the elements of the chemist, cannot be split up into simpler characters. I do not pretend that my formulæ of heredity describe the numerical results obtained by jumbling a lot of my elemental units together, any more than you pretend that the Law of Ancestral Heredity describes the phenomena exhibited by my units when dealt with separately."

* Davy '08.

† This illustration may at first sight appear not to be strictly parallel. A chemist reading it would think that what was going to be shewn was that the unit "dark-eye-and-coloured-coat" was resolvable into "dark-eye" and "coloured-coat," whereas, of course, what is really going to be shewn is that the units "dark-eye and coloured-coat" are of two quite distinct kinds. The true parallel to my case is the idea of the element before Davy's time. Amongst the things that were classed as elements, some really were undecomposable (cf. the homozygous forms), while others—the alkalies—were decomposable (cf. the heterozygous).

The point that I wish to bring home to the reader is that the statement that Mendelism deals with units, and biometry with masses, is not merely a brief summarising statement which pleases the mind, but that it has an actual meaning in relation to the facts: that is to say, that the limits of these units are not set by the imagination, but are discovered by experiment.

4 (c). *Examples of the confusion between physiological and statistical Laws.*

Having spoken this much on the difference between physiological and statistical Laws of heredity I propose to consider a few cases where failure to perceive this difference has led to confusion. As I have already referred to Castle's case I will finish with it before I proceed to others: he says* "The foregoing results show very clearly that albinism conforms in the mode of its inheritance to Mendel's law of heredity. The fact, however, must not be overlooked that a somewhat *different explanation of its inheritance*† has recently been given, based on Galton's "law of ancestral heredity" I shall not at this time enter into a detailed discussion of Galton's hypothesis, which was an entirely rational one *in the form in which it was originally proposed*,† and quite in harmony with the phenomena of *gametogenesis*† as then interpreted. I have shown, elsewhere[4] by a specific test in the case of mice, based on the observations of von Guaita, that Galton's law *fails to account for the observed facts*† concerning the inheritance of albinism, but that Mendel's law does this perfectly. Nevertheless Darbishire, likewise dealing with albinism in mice, though admitting that certain of his results are not in disagreement with Mendel's law, is

* Castle 05, p. 16. † Castle, 03, p. 231.

inclined rather to interpret the phenomena *on some such hypothesis as that of Galton.*"[5]

The numerals refer to the words in italics preceding them.

1. This shews that Castle followed me in confusing Galton's Law with the Law of Contribution.
2. Here we see that Castle has started on a right track: he has perceived that Galton's Law in the sense in which I used it, meaning the Law of Contribution, is not the same as that Law as first enunciated.
3. And yet he thinks that in its original form it was in harmony with the phenomena of gametogenesis as then interpreted, whereas it seems to me that the chief characteristic of a statistical Law is that it is independent of any theory of gametogenesis whatsoever.
4. Of course it does: because it does not attempt to. What he means is that the Law of Contribution attempts *and* fails: and this is quite true.
5. Here again as in (2) we see light breaking in on the confusion between Galton's Law and the Law of Contribution. Castle sees that the theory of heredity I had in mind is not quite the same as Galton's: I have shewn (p. 15) exactly how it differs from it.

I will now refer to a case in which the confusion between Galton's Law and that of Contribution is complete.

In 1904 I wrote:* "*I do not propose to discuss here the difference*[1] between Mendelian principles and the *statistical conception of inheritance*,[2] but to consider one part of the hypothesis put forward by Mendel, which is at variance with *Galton's theory*.[3] I refer to the phenomenon of segregation. We have seen what Mendel says. But this is flatly contradicted by the *Galtonian generalization*,[4] according to which the greater number of generations a

* Darbishire :04*b*, p. 9.

given hybrid is from the first hybrid . . . . . , the fewer pure recessive and dominant forms is it likely to produce when mated with another hybrid of its own generation."

1. For the very good reason that I did not then understand it: as I do now.

If "Law of Contribution" is put in the place of the terms 2, 3, and 4, this passage is quite true. As it stands, it is nonsense.

Perhaps, after all, the most complete example of this confusion is to be found in Castle's writings: it occurs in his paper on Galton's and Mendel's Laws: he gives a table * to shew the difference between the Mendelian and Galtonian prediction of the number of albinos produced by crossing Japanese waltzing mice with albino, together with the actual numbers that occurred in von Guaita's well known experiment.†

I give the top 2 lines of the table.

Generation.	Total Young.	Mendel's Law. Calculated No. of Whites.	Observed No. of Whites.	Galton's Law. Calculated No. of Whites.
II.	28	0	0	14

Substitute Law of Contribution for Galton's Law, and the idea conveyed by the Table is sensible and true.

I will refer to one more passage in which Galton's Law is used in the sense of the Law of Contribution: "Nach *Galtons* Theorie muss jede Gamete, welche von einem Individuum produziert wird, imstande sein, ***alle*** Merkmale der Sippe, welcher es angehört, auf die Nachkommen zu übertragen; es ist unmöglich, dass gewisse Gameten für immer von der Übertragung gewisser

* Castle '03, p. 231.

† Guaita '98 :00.

Merkmale ausgeschlossen werden."* Here "Galtons Theorie" is made to refer not merely to the individual but to the gamete borne by it, and the expression as here used means nothing more nor less than the Law of Contribution. It is true that Galton himself tentatively suggested,† when he formulated his Law, that it might become applicable to the individual.‡ But his Law as it stands is a statistical Law true of masses of units; and when a physiological theory of heredity, as in the above quotation, is spoken of as "Galtons Theorie" it is high time that a new term is invented to describe it: I have proposed the "Law of Contribution."

Nothing could be more fatal to profitableness of discussion than that two such profoundly different things as Galton's Law and the Law of Contribution should go by the same name.

So long as physiological are not clearly distinguished from statistical Laws of heredity, biologists will continue to slide from meaning a physiological to meaning a statistical one: and the transition will be unconscious because the term by which they denote these two different things is the same—namely Galton's Law. Progress in the study of heredity will be slow as long as this confusion prevails. For so long as it prevails we shall continue to hear the insensate statement that ancestry *makes a difference.* Of course it makes a difference—in the mass; which it is the business of the biometrician to measure and of the Mendelian to account for. Anyone who proclaims that his results prove that ancestry makes a difference, without making it clear whether he has in mind a physiological or a statistical theory, is drawing a conclusion which is meaningless. For his conclusion to have

* Lotsy '06, p. 152.
† See Appendix A.
‡ Galton '97, p. 403.

a meaning he must make this clear. If he is referring to the former, he is declaring for the Law of Contribution; if to the latter, for the Law of Ancestral Inheritance.

When the Mendelian says that ancestry does not make a difference, he is not denying the validity of the Law of Ancestral Inheritance but the Law of Diminishing Individual Contribution. At least I think this is the correct attitude.

And I cannot bring myself to agree with Bateson when he says that facts once describeable by Mendel's Law are permanently removed from the operation [sic] of the Law of Ancestral Inheritance, unless all that he means by this statement is that when we have gained this deeper knowledge of certain hereditary phenomena their further treatment by the method of the correlation table will not increase our knowledge of them. I should like to think that this is all he means: but his writings prevent me: for he imputes to upholders of Pearson's Law belief in the Law of Contribution:* yet on the next page he shews that he has not confused the two, by saying that the Law of Ancestral Heredity "*does not directly attempt to give any account of the distribution of the heritage among the gametes* of any one individual." I do not know whether Bateson *still* holds that Mendel's Law is antagonistic to the Law of Ancestral Inheritance as well as to the Law of Contribution. If he does, I do not understand on what grounds. Pearson has investigated the relation between the two and concludes "that in the theory of the pure gamete there is nothing in essential opposition to the broad features of linear regression, skew distribution, the geometric law of ancestral correlation, etc. of the biometric description of inheritance in populations."†

* Bateson, :02, p. 21, second half.

† Pearson, :03*b*, p. 86.

And no flaws in the argument of my paper on the supposed antagonism of Mendelian to biometric theories have been pointed out to me; in fact, Correns* and Giard† have expressed their agreement with it.

I feel most strongly that so long as we confuse physiological with statistical Laws of heredity we are wandering in the dark: we cannot know in what direction our studies are leading us, whether we are establishing correlations among the leaves or are digging among the roots. It may or may not be that what we learn by the former method is all that we shall ever know, and that we shall find nothing by our digging; but be this as it may, I hold that it is essential to progress in discovery, no less than to clearness of thought, that we should know which of the two we are doing.

4 (*d*). *Description of a method of dealing with the material of a breeding experiment in such a way that the data obtained may be used to test the validity both of Mendel's and Pearson's Law.*

When I had finished my last paper on my hybridization experiment with mice‡ I was still of the opinion that Mendelian and biometric Laws of heredity were mutually exclusive, and that if I could discover which of the two was true, I should be making a forward step in our knowledge of heredity. I therefore devised an experiment which was destined to settle this question; and wasted a year in carrying it out. As soon as I discovered the true relation of the two Laws I devised a method of dealing with my experiment, of such a kind that the results could be utilized by the Mendelian or the bio-

* Correns, :05, p. 43.
† Giard, :05, p. 22.
‡ Darbishire, :04*a*.

metrician to test his own particular Law; for the stringency with which the mice were selected in the previous part of the experiment rendered the results useless for anyone who wished to test the Law of Ancestral Inheritance by them.

What was wanted was some device to ensure the random mating of the mice, and, at the same time to ensure the possibility of tracing all the ancestors and all the offspring, in fact, all the relations of every degree of every individual mouse; the second condition had been fulfilled in the previous part of my experiment; but the first had not, because the different kinds of mice had been rigidly selected.

The method by which I mated the mice at random was very simple. I wrote the catalogue-name of each mouse on a counter; then I put the counters representing female mice into one hat and those representing males into another: all that remained to be done was to draw out at random a counter from the 'female' hat and similarly one from the 'male' hat, and to mate the actual mice represented by these counters. As I have said, this method enables one to test the Law of Ancestral Inheritance and Mendel's Law. As far as the first is concerned it is the most perfect conceivable; but for the second it is clumsy and involves unnecessary labour: because what is aimed at in a Mendelian experiment is the discovery of the properties of character-units, as far as they can be discovered by determining the specific results of their union with similar and dissimilar character-units. Now, some particular combination of characters may turn up very seldom by the method of random union; and if one wishes to discover the result of such a combination one has to wait until the drawings from the hat give it. One is in the position of an observer, and if one wishes to

attain the knowledge of the result of such a combination quickly and in large numbers, the random mating and the counters must be discarded and one must deal *experimentally* with the material, isolating the individuals the properties of whose character-units one wishes to determine.

I had planned at the beginning of last year (1905), to do the same experiment with peas by mating at random peas with green round seeds (Eclipse) with peas with yellow wrinkled seeds (British Queen), and with each other; and had already sown the seed; when it occurred to me that I need not have done so. We know* the result of crossing a yellow wrinkled with a green round pea, and of their mating *inter se:* so that all that is necessary is to start with a hat containing equal numbers of yellow and green counters representing pistil parents, and a hat with similar contents representing pollen parents, and to mate the contents at random, the result of each of the 3 possible unions, $g \times g$, $g \times y$, $y \times y$, being known by previous experiment. And the result of the matings of the various kinds of offspring can be predicted from the knowledge, which we have, of their gametic constitutions. Thus, for example, in F_2 a yellow resulting from the union yellow × yellow† will produce only yellow when mated with green; but a second yellow (indistinguishable by outwardly observable features from the first) produced by the union yellow × green will produce half yellows and half greens when mated with green; while a green of what ancestry soever will always produce green when mated with green.

Ex hypothesi Mendeliano it is possible to predict the result of these unions for however many generations

* Hurst :04.

† These colours refer to the gametes.

through which the experiment is continued, because in its simple form that Law states that the result, for example, of $DR \times DR$ will always be the same whether the mating of hybrids takes place in F_1 or F_2. The Mendelian hypothesis in this simple form may or may not be right; and I for one think that it is not. But this does not damage my argument. My point is that you *can* determine the properties of the hybrids in different generations —supposing that they are not the same in all; and having acquired this knowledge you can then return to the counters and see whether the result of mating your material at random can be described by the Law of Ancestral Inheritance. What I want to make clear is that the knowledge of heredity acquired by the Mendelian is deeper; is nearer the phenomena themselves, than is that acquired by the biometrician; and is such that the latter if he is inclined can use it as material with which to test the Law of Ancestral Inheritance, without the labour of conducting a breeding experiment.

Having devised my method, therefore, I discovered that it was unnecessary to use it. So I abandoned it; in the case both of the mice and of the peas I am now investigating the properties of the various kinds of individuals in various generations in both cases, accumulating information of a physiological nature) which will be available to the biometrician for use in testing the Law of Ancestral Inheritance.

4 (c). *Why do white sheep eat more than black ones?*

I was asked the other day this well-known riddle: and as I had forgotten the answer I was told it: "Because there are more of them."

The supplying of the answer never provokes a laugh, yet the relation between it and the question is full of

interest. Let us discuss it. When you ask the riddle you do not say that you are not referring to individual white and black sheep, but the man of whom the riddle is asked *invariably* thinks that you are: in attempting to answer it, the ideas that rush through his mind may either take the form of seeking for some pun on the words or perhaps for some humorous quotation in which they appear; and so forth: or, what usually happens, he thinks that as a matter of fact a white individual *does* eat more than a black, and (if he is a biologist) he may be trying to think of some physiological explanation of the fact, in connection possibly with the well-established relation between pigmentation and the getting rid of waste products.

In the answer he is told that the amount eaten by the sum-total of white sheep as compared with that eaten by the sum-total of black sheep is the subject under discussion; and not any peculiarities of ingestion, digestion, or egestion associated with whiteness as compared with blackness.

If the antithesis between truths about masses, and truths about individuals which constitutes the point in this riddle were more widely and more clearly perceived than it is to-day, there would no longer be that confusion in the minds of most biologists which prevents them seeing the profound difference that exists between a physiological Law like Mendel's, which is true of units, and a statistical one like the Law of Ancestral Inheritance, which is true of masses. All intending students of heredity should be asked this riddle; and if they cannot detect the fallacy in it they should be declared unfit for their intended task.

The similarity between the impression called into existence in the mind by asking the question and Men-

delism; and that between the idea conveyed by the answer and the Law of Ancestral Inheritance does not lie only in the fact that while the question and Mendelism deal with individuals, the answer and the Law of Ancestral Inheritance refer to masses. The idea implied in the question is like Mendelism, because it suggests what Mendelism effects, the discovery of a hitherto unsuspected order in familiar phenomena; while the truth conveyed in the answer is like the biometric treatment of heredity, because it is the accurate statement of a relationship that you already know to exist. Everyone knows that the sum-total of children are more or less like the sum-total of their parents; the biometrician accurately measures the degree of this resemblance.

The answer you expect is physiological. The answer you get is statistical.

APPENDIX A (p. 26).

It is interesting to enquire what Galton himself said when he formulated his Law, on the subject of its applicability to individual cases. He said ('97, p. 403): "It should be noted that nothing in this statistical law contradicts the generally accepted view that the chief, if not the sole, line of descent runs from germ to germ and not from person to person. The person may be accepted on the whole as a fair representative of the germ, and, being so, the statistical laws which apply to the persons would apply to the germs also, though with less precision in individual cases. Now this law is strictly consonant with the observed binary subdivisions of the germ cells, and the concomitant extrusion and loss of one-half of the several contributions from each of the two parents to the germ-cell of the offspring." Mark his words, ". . . . though with *less* precision in individual cases"—the italics are mine. If one were referring to Galton's Law (in the form in which it is true of masses only) one would say, ". . . . without applying at all to individual cases": and if to the Law of Contribution, ". . . . with *absolute* precision to individual cases." But I may be interpreting this wrongly, for the "less" may refer not to the difference between population and individual, but to the difference between person and germ. And, in fact, I think the following quotation from the previous page ('97, p. 402) justifies us in concluding that Galton conceived his Law as being true solely of masses without being true of the component individuals. "The neglect of individual prepotencies is justified in a law that avowedly relates to average results: . . . ." At any rate it simplifies matters very much to consider that Galton's Law as he formulated it is true of masses only, and not of their component units: for if we do not, we have to keep *three* laws distinct in our minds.

1. Galton's Law as he formulated it: true of masses, but also, though with less precision, of individuals. Statistical and Physiological.

2. Galton's Law: true of masses only. Statistical.
3. The Law of Contribution: true of units. Physiological.

APPENDIX B to p. 5.

There is nothing, of course, in the word 'contribute' to definitely signify that the thing which is contributed is the same as that which contributes: in fact, in the everyday usage of the term this is hardly ever the case. But it is reasonable to hold that Galton's Law is the generalisation that like contributes like and not unlike: and it is certain that Galton himself meant this, as the last words of his illustration of particulate inheritance readily shew ". . . . each piece of the new structure is derived from a corresponding piece of some older one, as a lintel derived from a lintel, a column from a column, a piece of wall from a piece of wall." ('89, p. 8).

APPENDIX C to 4 (*b*) *pp.* 19—23.

There is an apparent paradox, in the ideas just expressed, about which I think it is necessary to say a few words, in case the reader should detect it himself and think that it had not occurred to me.

I have said that biometry deals with masses and Mendelism with units; but I have also said that the biometrician exceeds his proper limits when he goes beyond the boundary of a unit, while the Mendelian is concerned with the mutual properties of numerous units: in other words the sphere of the biometrician is within the unit while that of the Mendelian is outside it.

The fundamental idea on which the Law of Ancestral Inheritance is based is that set forth in the quotation from Pearson on p. 4 of this essay; it is that a knowledge of the characters of the parents does not enable us to predict the character of the offspring in individual cases.

The fundamental idea in Mendelian theory is that the ascertainable gametic characters of the parents *do* enable us to predict the character of the offspring in individual cases.

How are these two diametrically opposite ideas about heredity to be reconciled? The answer which most naturally suggests itself is that the biometrician happens to have dealt with cases about which it was impossible to predict in individual cases; while the Mendelian happens to have dealt with cases in which prediction was possible. This answer presupposes the existence of two sets of phenomena in heredity, those about which it is possible to predict, and those about which it is not. Now let us grant for the moment that the Mendelian theory (which I think by no means proven yet) that the characters of an organism consist of a number of separate character-units is true. What relation, if any, do the two sets of hereditary phenomena—the predicable and the non-predicable bear to these units? Just this. The non-predicable phenomenon is the incomplete correlation between the degree in which any character x is exhibited by a parent, in a single case, and the degree in which that same character is exhibited in its child. The predicable phenomenon is the result of the union of x with x, or of x with y.

Chemistry furnishes a parallel. The chemist cannot predict the rate at which any given atom in a litre of oxygen is travelling; he can only deal with 'statistics of average conduct'; but he can predict the result of passing an electric spark in a vessel containing oxygen and hydrogen. Yet he who deals with the properties of the elements may be said to deal with units, and he who deals with the component atoms—and one can only deal with them in large numbers—may be said to deal with masses.

It is true that the biometrician possesses the only means of

measuring the "intensity of heredity" in a non-predicable case, but it seems to me that to extend the application of these means to predicable cases is fallacious. If the true function of the biometrician is to give us statistics of average conduct where we *cannot* predict individual conduct it seems to me that to deal by the biometric method with cases where we *can* is not only unprofitable, but likely to lead men to think that where there are two methods dealing with the same material of which the one can predict while the other cannot, the latter is fallacious. Whereas if the biometrician confined himself to the non-predicable and the Mendelian to the predicable, the general conclusion would be that each had his proper sphere—which indeed, in my belief, he has. I do not set forth these views in any spirit of dogmatic certainty; and nothing could please me less than that they should go unchallenged by anyone who believes me to be mistaken.

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Manchester Memoirs, Vol. l. (1906), *No.* 12.

XII. Notes.—On an allotropic form of Arsenic and On the Estimation of Arsenic when in Minute Quantities.

By WILLIAM THOMSON, F.R.S.E., F.I.C.

Read March 13th, 1906. Received for publication June 26th, 1906.

I wish to call attention to a peculiar allotropic form of arsenic which is produced when that substance is sublimed and rapidly condensed in vacuo or in presence of an inert gas such as hydrogen or carbon dioxide. When in small quantities it condenses as a white film which rapidly becomes black. This blackening is brought about almost instantaneously by the light from a burning magnesium wire, or by bright daylight, but it takes place rather rapidly by ordinary gaslight, and the whiteness is not preserved when the film is kept in the dark. It continues white for a longer time when kept at the ordinary temperature of the atmosphere than when heated to, say, 100°C. It becomes black in about 20 minutes when cold and in the dark, and in about five minutes in diffused light.

A platinum wire covered with a thin layer of glass upon which ordinary arsenic was laid was enclosed in a glass bulb. A vacuum was made in the bulb by a Topler vacuum pump till no more air could be extracted, and left 24 hours over phosphoric anhydride. The pump was again worked and proved that no leakage of air into the bulb had taken place. A current of electricity was then passed through the wire to heat it to redness, and so

August 14th, 1906.

evaporate some of the arsenic, which condensed on the top of the bulb as a white film, and rapidly became black as it did when sublimed in an atmosphere of hydrogen or of carbonic anhydride.

It occurred to me that this blackening action in the dark may be due to some obscure radiations which are everywhere present, and some experiments were made by enclosing these films in thick lead tubes, but they appeared to blacken when so enclosed just as rapidly as when enclosed in an ordinary wooden pencil case. Nevertheless these white films may prove of interest to the physicist, as some means may be found for preserving their whiteness, and so making it possible to determine the presence of certain radiations as yet unknown.

On placing tubes containing these mirrors in liquid air they remained white for 5 hours, during which time they were immersed. The test mirror which was momentarily withdrawn from the liquid air from time to time became blackened, whilst the others which were immersed and in the dark, remained white.

I find that this form of arsenic has been studied by several observers,* but more especially by Hugo Erdmann and Max von Unruh who find that it is more volatile than the black form (which our observations confirm). They say it is soluble in carbon disulphide, and whilst in solution it is not affected by light, but may be recovered as a yellow deposit on evaporation of the solvent, and it is then very sensitive to all kinds of light. On being left in solution

*Schuller, *Math. u. naturn. Ber. aus Ungarn*, 1889, vol. 6, p. 94.

Retgers, *Zeit. anorg. Chem.*, vol. 4, p. 403—409, and vol. 6, p. 307–320.

McLeod, *Chemical News*, 1894, vol. 70, p. 139.

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in a stoppered bottle a reddish form of arsenic is produced which gradually falls from solution.

Improvement in the cooling method for condensing the arsenic mirrors in arsenic determinations.

The results obtained by the method which I formerly adopted of running a stream of cold water over a piece of tissue paper covering the drawn out portion of the tube, leave something to be desired, especially in the estimation of exceedingly minute quantities of arsenic, for the water at the edge of the paper next the flame does not give an exceedingly sharp line of cooling surface, and we have improved on this by taking a piece of block tin tube 8 inches long, having an internal bore of $\frac{3}{16}$ths inch. This tube is flattened so that the internal measurements become $\frac{1}{2}$ inch × $\frac{1}{8}$th inch, and it is bent downwards in a curve about $\frac{1}{2}$ inch from the end. Two holes are made in the edges of the flattened tube opposite each other 2 inches from the bent end, and a thin silver-foil plate soldered over one of these holes. This plate is perforated by drilling in it a hole of a diameter sufficient to allow the drawn out portion of the largest sized glass tube to fit it. The tube is passed through the silver plate to the point at which the mirror is to be formed, and the water turned on through the tin tube, which is connected with the main by a thin rubber tube. A gentle flow of water is kept running through this tube, and it is found that if the length of the bent end of the tin tube is properly adjusted the water does not escape at the silver plate or the hole in the tin tube opposite, even when the tubes fit comparatively loosely. By this device $\frac{1}{160}$th of a grain per gallon of arsenic trioxide when working on 50 c.c. of the solution can be readily seen as a distinct narrow

metallic ring, and the larger mirrors are more evenly deposited.

Influence of nitrous compounds on the estimation of minute quantities of arsenic.

I observed in making tests by the electrolytic as compared with the Marsh-Berzelius method that in some cases considerably larger mirrors were obtained by the latter than by the former, and on investigation it turned out that this difference was due to the fact that all the nitrous compounds used for the destruction of the organic matter had not been removed. To be certain of the results by the electrolytic process it becomes necessary that the sulphuric acid should be evaporated to its fuming point, diluted with twice its volume of water and again evaporated to the fuming point, and this should be again repeated to ensure the elimination of all of the nitrous compounds, so that the proper size and depth of arsenic mirrors may be obtained by the electrolytic process in the time allowed for the test. With the Marsh-Berzelius method the presence of some nitrous compounds does not so materially affect the result.

With a view of studying the influence of nitric acid in the electrolytic apparatus, a dilute solution equivalent to half a c.c. of the ordinary strong acid of commerce (0·48 grm. HNO_3) was added separately to an electrolytic apparatus—the first with a lead, the second with a zinc, and the third with a graphite kathode. This acid was gradually converted into ammonia in each apparatus, the amount of which was then determined. A current of 3 amperes was employed, and each apparatus was worked in series. After 45 minutes the ammonia in the solutions in the various kathode chambers was determined, and the following results obtained :—

	HNO_3 reduced to NH_3 in 45 minutes.
*Lead kathode	0·22 grm.
Zinc „	0·21 „
Graphite „	0·10 „

This shews that the rate of decomposition was about the same with lead and zinc kathodes, whilst the graphite kathode only exerted about one half the reducing action.

Influence of ½ *c.c.* HNO *on the detection of arsenic in the form of* As_2O_3 *by the electrolytic apparatus when using kathodes of different substances.*

A solution was prepared containing 0·000,000,83 grms. of As_2O_3 in each cubic centimetre. 1 c.c. is equivalent to $\frac{1}{1000}$th of a grain of As_2O_3 per gallon when using 50 c.c. of the liquid; 10 c.c. of this solution were mixed with ½ c.c. of HNO_3 and introduced into the apparatus, using kathodes of different substances. The kathodes used were graphite, lead, cadmium, zinc, and iron.

After the current (3 amperes) had run for 45 minutes the arsenic mirrors obtained were approximately equivalent to the following c.c. of the standard solution:—

(The mirrors obtained when HNO_3 is present are very irregular, and only admit of an approximate estimation. In each case the number of c.c. is given as the nearest whole number, and the figures represent averages from a number of experiments.)

	c.c. of standard solution.
Graphite	1·0
†Lead	6·0
Cadmium...	2·0
Zinc	4·0
Iron	2·0

* This lead kathode was made and kindly given to me by Mr. J. E. Hackford, of Nottingham. The lead used having been purified from traces of arsenic and antimony by him by fusing with sodium, and afterwards stirring up with pure fused sodium chloride while the lead is still at a high temperature.

† Another lead kathode made by ourselves gave practically the same result, although the results shewed some difference.

It is remarkable that the arsenic acid was reduced by each of the different kathodes to the condition of arseniuretted hydrogen in presence of nitric acid or some nitrous compounds, some of which remained intact after the experiment, and that lead was the most efficient.

Influence of different kathodes on arsenious acid.

For this series of experiments a standard solution was prepared containing 0·000,000,715,4 grm. of As_4O_6 per c.c. 1 c.c. is equivalent to $\frac{1}{1000}$th of a grain per gallon when working on 50 c.c. of the solution, and 10 c.c.'s of the standard solution were used for each experiment, which was carried out during 45 minutes, in each case with a current of 3 amperes.

The mirrors obtained during 45 minutes were equivalent to the following c.c. of the 10 c.c. of the standard solution taken—the mirrors obtained with a pure zinc kathode during 45 minutes being taken as the standard :—

	C.C. of standard solution.
Graphite	9
Lead	10
Cadmium	3
Zinc	10
Iron	8

Influence of different kathodes on arsenic acid solution.

Each c.c. of the standard solution used contained 0·000,000,83 grm. As_2O_3 ($=\frac{1}{1000}$th of a grain of arsenic trioxide per gallon when using 50 cc.).

The experiments were made as before with 10 c.c. of standard solution, and the time and electrical current as above mentioned. The resulting mirrors obtained were as follows in equivalents of c.c. of the standard arsenic trioxide solution :

	c.c. of standard solution.
Graphite	1
Lead	9·0
Cadmium...	2·5
*Zinc	9·0
Iron	5·0

The iron electrode was made by wrapping closely round a glass tube pure fine iron wire, such as is used for making standard solutions.

Part of the arsenic seems to combine with the iron and to form such a combination that it is not eliminated as AsH_3 when the current is passed. When iron which has previously been used in an arsenic test is used immediately afterwards with acid free from arsenic, it gives a perfect blank after working for 45 minutes, but on being left till the surface oxidises, and then repeating the test with pure acid, a mirror of arsenic is obtained.

Insensitive zinc in the Marsh-Berzelius apparatus.

When zinc contains small quantities of iron, etc., it becomes insensitive when used in the Marsh-Berzelius apparatus, *i.e.*, it gives off hydrogen free from arseniuretted hydrogen, and even when minute quantities of arsenic are added it still fails to produce a mirror. Chapman and Law (*Analyst*, January, 1906, vol. 31, p. 358), say that it is due to the hydrogen being given off at a lower electrical supertension, owing to the metal existing as an impurity, being capable of liberating hydrogen at such lower supertension. They say, however, if 2 grammes of cadmium sulphate be added to the solution, that the insensitive zinc becomes sensitive. It seems remarkable that a metal such as cadmium which liberates hydrogen at a lower supertension than zinc

* In working with As_2O_3 the 9 parts would be represented by the standard mirrors as to for that time of the experiment.

should have the effect of doing away with the influence of the iron; in other words that the deposition of cadmium in a spongy condition on the zinc (which is the metal dissolved by the acid) should have the effect of raising the supertension at which the hydrogen is evolved, whilst the iron still remains and must presumably act as it did before the addition to it of a spongy covering. I have repeated his experiment with a sample of electrolytic zinc prepared by Brunner, Mond & Co., which contains a small proportion of metallic iron. It retains minute quantities of arsenic added to the sulphuric acid used in the Marsh-Berzelius apparatus. We added 2 grammes of cadmium sulphate to this zinc, as recommended by Chapman and Law, and tried experiments by introducing minute quantities of arsenic into the apparatus, with and without cadmium sulphate, but we failed to find that the cadmium sulphate made any difference. The cadmium was thrown down on the zinc, and the evolution of hydrogen much reduced thereby, but in our hands it remained as insensitive as it was before, although the strength of the sulphuric acid was increased so as to give about the same flow of hydrogen.

Chapman and Law mention, as I previously found, that magnesium is insensitive. I endeavoured to obtain magnesium free from arsenic and antimony, but failed. I took, however, the sample which contained the smallest quantity of arsenic, and made the test by dissolving completely 1·4 grammes of magnesium. The experiment was repeated with the addition to it of 0·2 gramme cadmium sulphate. In the latter case a slightly larger mirror was obtained than in the former. 4 c.c. of the standard arsenic trioxide solution was then added with 1·4 grammes of magnesium, and a mirror obtained equivalent to from 2 to 2½ c.c. On repeating the experiment with the addi-

tion of 0·2 gramme cadmium sulphate, a mirror equivalent to 4 c.c. was obtained.

I dissolved 0·5 gramme of nickel in 100 grammes of pure zinc, and tried this alone; it gave no mirror. I then added 4 c.c. of standard As_4O_6 solution, and it gave a mirror equal to that obtained when pure zinc is employed in the manner previously recommended by me.* On repeating this with the addition of 0·2 gramme of cadmium sulphate a mirror of about the same size was obtained. Even 1 c.c. of the standard solution (which is equivalent to 0·000,000,714 gramme of As_4O_6) gave the full mirror. Thus the zinc nickel alloy was quite as sensitive as pure zinc, although it dissolved much more readily in the sulphuric acid, and the addition of 0·2 gramme of cadmium sulphate made no difference in the size of the mirrors obtained.

We then made alloys of zinc with 0·5 gramme of cobalt, iron (in the form of wire), and copper respectively, and of these iron shewed the most marked retention of arsenic, for from 20 c.c. of standard arsenic solution a mirror equivalent to about 3 c.c. was obtained, whereas cobalt and copper gave mirrors corresponding with about 20 c.c. in each case.

The addition of 0·2 gramme of cadmium sulphate increased the sensitivity of the iron alloy, a mirror equivalent to about 10 c.c. being obtained from 20 c.c. of the standard arsenic solution.

It seems to me that the action of cadmium sulphate in rendering zinc and other metals more sensitive in the determination of arsenic in the Marsh-Berzelius apparatus must be further studied.

I have pleasure in stating that I am indebted to the care and ability which has been shewn by my assistant, Mr. Edwin Hopkinson, M.Sc. (Vict.), in carrying out these experiments.

* *British Food Journal*, vol. 4, nos. 44 and 45.

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XIII. Notes on the Palæarctic Species of Coal-Tits.

By FRANCIS NICHOLSON, F.Z.S., &c.

Received and read April 10th, 1906.

During a visit to London in 1903, I obtained eleven specimens of the Cyprian Coal-Tit (*Parus cypriotes* of Dresser), eight males and three females, which I have presented to the Manchester Museum. On comparing these birds with the series of Coal-Tits in the British Museum, I made the following notes, which may be of some interest to ornithologists.

In 1894, an excellent series of papers on the Coal-Tit (*Parus ater*) and its allies was published by the late Mr. J. P. Pražák in the *Schwalbe*, the Journal of the Ornithological Club of Vienna, and these essays were afterwards published by him in a collected form in a pamphlet of 44 pages, entitled, "Einige Bemerkungen über die Tannenmeise (*Parus ater*, L.) und ihr nahestehende Formen." He recognised the following races, and gave their full synonymy and geographical distribution.

1. Parus ater.
2. Parus ater britannicus.
3. Parus ater cypriotes.
4. Parus ater michalowskii.
5. Parus ater phaeonotus.
6. Parus ater aemodius.
7. Parus ater rufipectus.
8. Parus ater pekinensis.

August 15th, 1906.

I do not intend in the present short article to reproduce the whole of the synonymy so elaborately detailed by Mr. Pražák, but the following notes occur to me.

Mr. Pražák, in speaking of the typical Coal-Tit of Europe, which is *Parus ater* of Linnæus (founded on the Scandinavian species), includes Great Britain and Ireland as within its range. In proof of the last-named locality, he quotes Thompson's "Birds of Ireland," and Seebohm's paper on Irish birds in the *Ibis* for 1890 (p. 400); but in these instances the name of *Parus ater*, Linn., for the Irish bird was given by Thompson in ignorance that the British Coal-Tit was different from the typical continental form, and by Seebohm doubtless from conservative notions of nomenclature. The Coal-Tit of Ireland is the same as that of England and Scotland, viz., *Parus britannicus*, and is not *Parus ater*, which is the species of Scandinavia and the continent of Europe. *Parus britannicus* is the resident species of the British Islands, although the true *P. ater* is said to occur occasionally in England. I have never myself seen an English specimen of *P. ater*, and there is not one individual in the collection of the British Museum or the Manchester Museum, but that occasional specimens are to be met with cannot be doubted. Considering the hordes of tiny Goldcrests that annually migrate to the eastern coasts of Britain, there is nothing wonderful in the appearance of an occasional Coal-Tit from Scandinavia. Our British bird has an olive-brown back in its full winter plumage, but as this plumage gets worn during the breeding season, it becomes more grey, and it then resembles in some slight degree the grey-backed *P. ater*, but this latter species is blue-grey both in summer and winter, and the comparison of a series shows that the two birds are really quite distinct.

I agree with Dr. Bowdler Sharpe's observations on

the two species ("Handb. Brit. Birds," vol. 1, p. 137), where he remarks that, although in worn plumage during the nesting season, *P. britannicus* may lose some of its olive tint through the wearing away of the edges of the feathers, yet there is never any real difficulty in distinguishing our native bird from the true *P. ater*, which can only be considered an occasional visitor to the British Islands.

Mr. Pražák has given the full synonymy of *Parus ater*, and assigns to it the following geographical range:—France, Portugal*, Spain, Italy, Sicily, Sardinia†, Switzerland, Belgium, Holland, Germany, Denmark, Norway, Sweden, Austria, Hungary, Bosnia, Herzegovina, Servia, Montenegro, Bulgaria, Greece, Macedonia, Asia Minor, Palestine, Poland, Baltic Provinces, Russia generally, throughout Siberia to Amur Land, Kamtchatka, Ussuri Land, Askold Island, Japan, and the Liu Kiu Islands.‡

He likewise includes the island of Formosa, and hereby recalls a ludicrous episode in the history of ornithology. In Horsfield and Moore's "Catalogue of the Birds in the Museum of the East India Co." (vol. 1, p. 373), a specimen of *Parus ater* from Formosa is recorded as having been presented by Mr. John Gould. This specimen has now passed into the collection of the British Museum, and is undoubtedly only an example of the ordinary *Parus britannicus*. It has been prepared by Mr. Gould's own hands, and is evidently a bird procured by him, or by Mr. William Briggs, at 'Formosa,' Sir George Young's beautiful place on the Thames, near Cookham. In middle age, so I am informed by Dr. Bowdler Sharpe, who used frequently to meet him,

* Now separated as *Parus vicinus*, mihi.

† Now separated as *Parus sardus*, Kleinschmidt.

‡ Now separated as *Parus insularis*, Hellmayr.

Mr. Gould was a constant visitor to 'Formosa,' when Mr. De Vitré lived there. The head-gardener, Briggs, was a first-class naturalist, and was always on the look-out for specimens for Mr. Gould, when he was writing his "Birds of Great Britain." This is no doubt the way in which the specimen of *P. ater* from 'Formosa' came into Mr. Gould's possession, and was afterwards given by him to the Indian Museum.

It will be noticed further on that the Coal-Tit of Japan has been separated recently by Mr. Hellmayr under the name of *Parus insularis* (*vide infra*, p. 8).

In Mr. Dresser's "Manual of Palæarctic Birds" (vol. 1, 1902, pp. 164—167), *Parus ater* is recognised, with 2 sub-species, *P. britannicus* and *P. cypriotes*. *P. michalowskii* is united to *P. phaeonotus*, and *P. rufipectus* is made a sub-species of the latter bird, whereas, in my opinion, it belongs to the long-crested section of the Coal-Tits, being closely allied to *P. pekinensis*, which Mr. Dresser unites with *P. ater*.

Dr. Bianchi published in 1902 a very useful "Hand-list" of the *Paridae* in the *Annuaire* of the Petersburg Museum, and this was the foundation of Dr. Bowdler Sharpe's synopsis of the family, in his "Handlist of Birds" (vol. 4, 1903).

In 1903 appeared part 18 of "Das Tierreich," in which Dr. Hellmayr monographed the Paridae, with all that care which he bestows on every one of his undertakings. It is a very fine piece of work, and leaves little to criticise, although the system of nomenclature may not commend itself to all of us.

Mr. Hellmayr places the following species and sub-species in his sub-genus *Periparus*.

1. *Parus rubidiventris* Blyth.
2. *Parus rufonuchalis* Blyth, with two sub-species.

a. P. rufonuchalis rufonuchalis Blyth.
b. P. rufonuchalis beavani (Jerdon).

3. *Parus melanolophus* Vigors.
4. *Parus ater* Linn, with 11 sub-species.

a. P. ater britannicus Sharpe & Dresser.
b. P. ater cypriotes Dresser.
c. P. ater ater Linn.
d. P. ater insularis Hellmayr.
e. P. ater pekinensis David.
f. P. ater michalowskii Bogd.
g. P. ater atlas Meade-Waldo.
h. P. ater phaeonotus W. Blanford
i. P. ater aemodius Hodgson.
k. P. ater rufipectus Severtzoff.
l. P. ater ledouci Malh.

Mr. Hellmayr places *P. rufipectus*, Severtz, and *P. aemodius*, Hodgs., in the same section, on account of the ochre-yellow or pale cinnamon colour of the under surface, distinguishing the latter by its smaller size and long crest. In *P. aemodius* the crest-feathers are certainly abnormally developed for a member of the genus *Parus*.

P. rufipectus, Severtz. Of this species I examined several specimens on my last visit to the British Museum, from the Seebohm and Menzbier collections. In the same year (1873), Severtzoff gave two names to this species, *Parus piceae* and *P. ater* var. *rufipectus*. The former of these, being a *nomen nudum*, has been disallowed by subsequent authorities, and the name of *rufipectus* insisted upon. This method is perfectly correct, but in one respect it is to be regretted, as *rufipectus* does not invariably convey the impression of the colour of the bird's breast, which has often nothing especially *rufous* about it. A specimen from Thian Shan shews a faint

tinge of buff below, of much the same shade as in *P. pekinensis* and *P. insularis*, and from this I consider that *P. rufipectus* is more closely allied to *P. pekinensis*, both as regards colour and development of crest. The Thian Shan example is, in fact, a large edition of *P. pekinensis*. There are also examples from Transcaspia in the British Museum, some of them as pale below as the Thian Shan bird, while others are as cinnamon as *P. aemodius*.

P. ater of Europe appears to me to be the lightest in colour of all the pale-breasted group of which it is the type. It certainly has a whiter breast than most of the others.

P. pekinensis is, according to Mr. Hellmayr, an inhabitant of Southern Siberia, east of the Yenesei River and North China. Its distinguishing character is its very evident top-knot of long feathers. This feature is certainly developed in specimens from the far East to a greater extent than in typical *P. ater*, but the latter species is by no means devoid of a crest. Some examples of *P. pekinensis*, however, have nearly as long a crest as in *P. aemodius*, and it seems to me that the white spots on the wing-coverts are more conspicuous than in *P. ater*.

Of *P. pekinensis* I have examined several specimens in the British Museum. One from the Ussuri River (Lat. 48° N.: Dybowski) has quite long crest-feathers, and has a warm ochraceous-buff tint on the sides of the body, the lower back and rump being also washed with a light shade of ochraceous-buff. I have also examined, in the British Museum, one of the typical specimens given to the late Robert Swinhoe by Abbé David, and bequeathed to the Museum by Mr. Seebohm. It is from Pekin, and is a somewhat remarkable bird, not only on account of the tuft of long feathers on the crown, but from its rufescent under surface, wherein the breast and flanks are of a

distinct fawn-colour, approaching that of *P. rufipectus*, Severtz. The date of the specimen is December 14, 1867, so that it is in full winter plumage. I have elsewhere remarked on the difference in the colour of the under parts shewn by *P. rufipectus*, which has also a long crest similar to that of *P. pekinensis*, and has a rufous breast when in full feather, which fades to a sort of creamy fawn-colour in worn plumage.

I find that the same difference exists in *P. pekinensis*. A specimen from Kuatun, presented to the British Museum by Mr. C. B. Rickett, and obtained in May, is in very worn plumage, and is therefore very much paler below than the Pekin bird; it has an evanescent tinge of fawn-colour, and also an extraordinary crest of elongated plumes. No one examining either of these specimens of *P. pekinensis* could doubt the distinctness of the species from *P. ater*, and I can only suppose that Mr. Dresser had not examined these birds in the British Museum when he determined to unite the two species in his recently published "Manual of Palæarctic Birds."

Another specimen in the British Museum which I also consider to be *P. pekinensis* is a female bird from Chemulpo in Corea (C. W. Campbell: Seebohm Coll.), but those from the Gulf of the Amur, Ussuri Land, and Kamtchatka seem to be true *P. ater*, as do the birds from Krasnoyarsk. Siberian birds are slightly more fawn-coloured below, and shew some approach to *P. rufipectus*.

A specimen from the Gulf of the Amur River, collected by the brothers Doerries, and bequeathed by Seebohm to the British Museum, cannot be separated from *P. ater*, as it has no more crest than that species. The white bars on the wing, are, however, somewhat broader, and there is a distinct tinge of buff on the under-parts, especially on the flanks, though the rump is scarcely

tinged. A longer series of specimens from Eastern Siberia may prove that the Coal-Tit of the far East may be separable from the European species.

In the British Museum are birds from Irkutsk and Krasnoyarsk which I refer to *P. ater*, not to *P. pekinensis*, and I believe it to be a mistake to record the latter species from the Valley of the Yenesei.

P. insularis, Hellmayr, from Japan, is, as might have been expected, very closely allied to *P. pekinensis*, but there is a much more decided tinge of fawn-colour on the flanks, and it has the same conspicuous white spots on the wing-coverts. There is also a fulvescent tinge on the rump. Of the brown- or olive-backed section it is also difficult to write down the distinctive characters, but Mr. Hellmayr has given the characters which I enumerate below.

P. cypriotes, Dresser, is the darkest race of this whole section, the back being not so much olive-brown as dusky-grey, with an olive-brown wash. The smoky-brown tinge of the sides of the body is also much darker and less inclined to buff than in any of the other forms, and the black on the throat extends further on to the chest.

P. phaeonotus, Blanford, is a somewhat larger and browner bird, with a very little admixture of grey on the back. The sides of the body are of a pale fawn tint.

P. atlas, Meade-Waldo, from Morocco, is a representative of *P. phaeonotus*, but with the sides of the body darker and more smoky-brown.

P. moltchanowii is described by Professor Menzbier from the Crimea as a very distinct form, allied to *P. phaeonotus*.

P. michalowskii, Bogd. has a very stout bill, and has pale fawn-coloured flanks, but somewhat deeper in tint than *P. phaeonotus*. It is very closely allied to the latter species, but is a little darker brown.

P. britannicus, Sharpe and Dresser, is, when specimens in fresh plumage are compared, one of the most distinct of all the races of *P. ater*, remarkable for its olive-toned back, and the pronounced fawn-buff colour of the flanks. It has a very weak bill compared with that of its allies.

In 1905 was published part 3 of Dr. Hartert's monumental work "Die Vögel der paläarktischen Fauna," and he recognises the following forms of *Parus ater* and its allies.

Parus ater ater, Linn.—Europe to 65° N. Lat., apparently through N. Siberia to Kamtchatka. In Europe to the mountains of Spain, Italy, and Sicily.

Parus ater britannicus, Sharpe and Dresser.—British Isles.

Parus ater sardus, Kleinschmidt.—Sardinia.

Parus ater atlas, Meade-Waldo.—Atlas Mountains, Morocco.

Parus ater pekinensis, David.—N. China and Manchuria, probably west to the Yenesei Valley.

Parus ater aemodius, Hodgs.—E. Himalayas, eastwards to mountains of Kansu and Shensi in W. China.

Parus ater insularis, Hellm.—Japanese Islands (Yezo, Hondo, Liu Kiu Islands).

Parus ater rufipectus, Severtz.—From E. Thian-Shan Mountains to Issik-Kul.

Parus ater cypriotes, Dresser.—Cyprus.

Parus ater ledouci, Malh.—N. Algeria and N. Tunis.

Parus ater moltchanowii, Menzb.—Southern Crimea.

Parus ater derjugini, Sarudny and Loudon.—N. Armenia (Lasistan).

Parus ater michalowskii, Bogd.—Caucasus Mountains to Lenkoran.

Parus ater phaeonotus, Blanf.—Persia and S. Transcaspia.

Parus rufonuchalis rufonuchalis, Blyth.—Turkestan; Himalayas, from Gilgit to Gurwhal.

Parus rufonuchalis beavani, Jerd.—Nepal, Sikhim, eastward to W. China.

The conclusions arrived at by Dr. Hartert are, as will be seen by the above summary, confirmatory of those of Dr. Hellmayr. His work bears evidence of great care and judgment, and in future studies of the Palæarctic Coal-Tits, Hartert's essay must receive ample consideration.

Parus ater sardus, of Kleinschmidt, was described in the *Ornithologische Monatsbericht* for 1903 (p. 180).

Another species recently described is *Parus schwederi* (*Parus ater schwederi*, Loudon and Tschusi, *Orn. Jahrb.* vol. 16, p. 140, 1904). *Hab.* Livonia, Baltic Provinces. Dr. Hartert (*t.c.*, p. 356) does not consider this form separable from true *P. ater*.

The following is a list of the species of the sub-genus *Periparus*, or, as I prefer still to call them, *Parus ater*, and its immediate allies. This list is founded on the recent works of Hellmayr and Hartert.

Parus rubidiventer.

Parus rubidiventris, Blyth, *J. A. S. Beng.*, vol. 16, p. 445 (1847).

Lophophanes rubidiventris, Jerd, "B. Ind.," vol. 2, p. 274 (1863); Oates, "Faun. Brit. Ind., Birds," vol. 1, p. 58 (1889); Hartert, "Vög. Pal. Fauna," p. 362 (1905).

Parus (Periparus) rudiventris, Hellmayr, "Tierreich, Paridae," p. 74 (1902).

Periparus rubidiventris, Bianchi, *Ann. Mus. Zool. Acad. St. Petersb.*, vol. 7, p. 245 (1902).

Periparus rubidiventer, Sharpe, "Handl. B.," vol. 4, p. 326 (1903).

No white tips to the wing-coverts; under surface of body rusty-red on the centre of the breast and abdomen.

Hab. Himalaya Mountains, from Kumaon to Nepal.

Parus rufinuchalis.

Parus rufonuchalis, Blyth, *J. A. S. Beng.*, vol. 18, p. 810 (1849).

Lophophanes rufonuchalis, Jerd., "B. Ind.," vol. 2, p. 273 (1863).

Lophophanes rufinuchalis, Oates, "Faun. Brit. Ind., Birds," vol. 1, p. 58 (1889).

Parus (Periparus) rufonuchalis, Hellmayr, "Tierreich, Paridae," p. 75 (1902).

Periparus rufinuchalis, Bianchi, *Ann. Mus. St. Petersb.*, vol. 7, p. 246 (1902); Sharpe, "Handl. B.," vol. 4, p. 326 (1903).

Parus rufonuchalis rufonuchalis, Hartert, "Vög. Pal. Fauna," vol. 1, p. 361 (1905).

No white tips to the wing-coverts; breast and abdomen grey; throat and chest black. Size larger.

Hab. Russian Turkestan; Afghanistan; Mountains of Gilgit, N. Kashmir.

Parus beavani.

Lophophanes beavani, Jerd., "B. Ind.," vol. 2, p. 275 (1863, ex Blyth MSS.); Oates, "Faun. Brit. Ind., Birds," vol. 1, p. 59 (1889).

Periparus beavani, Bianchi, *Ann. Mus. St. Petersb.*, vol. 7, p. 245 (1902); Sharpe, "Handl. B.," vol. 4, p. 326 (1903).

Parus (Periparus) rufonuchalis beavani, Hellmayr, "Tierreich, Paridae," p. 75 (1903).

Differs from *P. rufinuchalis* in its smaller size, and in having only the throat black.

Hab. E. Himalayas, Nepal to Sikhim; to W. China (Kansu, Kokonor).

Parus melanolophus.

Parus melanolophus, Vigors. *P.Z.S.*, vol. 1 (1830), p. 23; Hartert, "Vög. Pal. Faun.," part 3, p. 362 (1905).

Lophophanes melanolophus, Hume, "Nests and Eggs, Ind. B.," p. 403 (1874); Oates, "Faun. Brit. Ind., Birds," vol. 1, p. 57 (1889).

Parus (*Periparus*) *melanolophus*, Hellm., "Tierreich, Paridae,' p. 76 (1902).

Periparus melanolophus, Bianchi, *t.c.*, p. 245; Sharpe, "Handl. B.," vol. 4, p. 326 (1903).

With distinct white spots on the median and greater wing-coverts; under surface of body grey.

Hab. N.-W. Himalayas, from Kumaon to Murree and Kashmir (Gilgit), Mountains of Afghanistan.

Parus ater.

Parus ater, Linn., "Syst. Nat.," vol. 1, p. 341 (1766); Pražák, *t.c.*, p. 4 (1894); Dresser, "Man. Pal. Birds," vol. 1, p. 164 (1902).

Parus (*Periparus*) *ater ater*, Hellmayr, "Tierreich, Paridae," p. 78 (1902).

Periparus ater, Bianchi, *t.c.*, p. 245; Sharpe, "Handl. B.," vol. 4, p. 325 (1903).

Parus ater ater, Hartert, "Vög. Pal. Fauna," part 3, p. 356 (1905).

With white spots on the median and greater wing-coverts; under surface of body whitish, with the sides distinctly washed with rust-colour; back ashy-blue; no distinct crest or top-knot; rump with a slight wash of olive-yellowish.

Hab. The whole of Europe and Siberia to the Gulf of the Amur and Kamtchatka.

In addition to its blue-grey back, the sandy-buff sides of the body are, in *P. ater*, decidedly duller in colour, and incline to smoky-brown. Scandinavian specimens are very clear blue-grey, but show a faint tinge of olive in the winter dress. A similar faint shade of olive is to be found on a few birds from the Vosges Mountains, obtained in October and November, but otherwise birds from Eastern France, Holland, and Belgium appear to be identical with those from Norway. Birds from Asia Minor are rather paler blue.

Parus insularis.

Parus ater (nec Linn.) Seebohm, "B. Japan, Emp.," p. 82 (1890), et auct.

Parus ater insularis, Hellm., *Orn. Jahrb.*, vol. 13, p. 36 (1902); Hartert, "Vög. Pal. Fauna," part 3, p. 359 (1905).

Parus (Periparus) ater insularis, Hellm., "Tierreich, Paridae," p. 75 (1903).

Periparus insularis, Sharpe, "Handl. B.," vol. 4, p. 325 (1903).

Mr. Hellmayr characterises this species as follows:—Similar to *P. ater*, and with the same bluish-grey back, but of a lighter and purer blue shade; rump as in *P. britannicus*, light olive-yellowish grey; under surface of body light olive-reddish-yellow, similar to that of *P. britannicus*.

P. insularis is of a very pure ashy-blue on the back. It only differs from *P. pekinensis* in its pale fulvescent under surface, which is rather lighter than in the latter bird. The crest is well developed, but does not form so distinct a tuft as in *P. pekinensis*. Some specimens are, however, hardly distinguishable.

Parus pekinensis.

Parus pekinensis, David, *Ibis*, 1870, p. 155.

Parus ater pekinensis, Pražák, *MT. Orn. Ver. Wien*, vol. 18, p. 32 (1894); Hartert, "Vög. Pal. Fauna," part 3, p. 358 (1905).

Parus (Periparus) ater pekinensis, Hellmayr, "Tierreich, Paridae," p. 78 (1903).

Periparus pekinensis, Bianchi, *t.c.*, p. 245; Sharpe, "Handl. B.," vol. 4, p. 325 (1903).

Similar to *P. ater*, with a blue-grey back, and large spots on the median and greater coverts, but distinguished by a very distinct top-knot or crest on the crown.

Hab. Corea. China (Pekin to Foh-kien).

Mr. Hellmayr's diagnosis is as follows: Similar to *P. ater*, but distinguished by an apparently well-developed crest on the hinder head. Nape-patch generally mixed with spots of blackish. Upper surface ashy-bluish, sometimes washed with olive-colour on the lower back. Rump rusty-yellowish; otherwise resembling *P. ater* in tint. Under surface whitish, the sides of the body washed with pale rusty-yellowish colour.

Mr. Hellmayr gives the range of *P. pekinensis* as Southern Siberia, east of the Yenesei, to China. As already mentioned in my note on *P. ater*, I cannot agree that the birds from the Yenesei in the Seebohm collection belong to *P. pekinensis*, but I consider that they are typical *P. ater*.

In the British Museum are specimens of *P. pekinensis* from Pekin (*David: Seebohm Coll.*); Kuatun (*Rickett Coll.*); Nikolaiesk (*Seebohm Coll.*); N. Ussuri Land (*Seebohm Coll.*); Chemulpo, Korea (*Campbell*), Kamtchatka (*Seebohm Coll.*).

Parus rufipectus.

Parus ater var. *rufipectus*, Severtz., "Turkestanskie Jevotnie," pp. 66, 134 (1873).

Parus ater rufipectus, Pražák, *MT. Orn. Ver. Wien*, vol. 13, p. 175 (1894); Hartert, "Vög. Pal. Fauna," part 3, p. 359 (1905).

Parus rufipectus, Dresser, "Man. Pal. Birds," vol. 1, p. 166 (1902).

Parus (*Periparus*) *ater rufipectus*, Hellmayr, "Tierreich, Paridae," p. 80 (1903).

Periparus rufipectus, Bianchi, *t.c.*, p. 245; Sharpe, "Handl. B.," vol 4, p. 325 (1903).

This species, as I have said before, seems to me to be a large race of *P. pekinensis*. It shows some affinity to

P. aemodius, but has not such a strongly developed crest, and it is decidedly larger.

It ranges from Eastern Turkestan to the Tian Shan Mountains. Some specimens in the National collection are apparently from Transcaucasia, but I could not decipher the Russian labels.

When in full plumage, *P. rufipectus* is easily recognised by its rufous-buff under-surface, the sides of the body being of the same colour as the breast: the wing-spots are also tinged with fawn-colour, and are not pure white as in *P. ater*. The back is dark blue-grey, and the fulvous shade on the rump is not particularly pronounced.

Parus britannicus.

Parus britannicus, Sharpe and Dresser, *Ann. and Mag. Nat. Hist.*, (4), vol. 8, p. 437 (1871); Dresser, "Man. Pal. Birds," vol. 1, p. 165 (1902).

Parus ater britannicus, Pražák, *MT. Orn. Ver. Wien*, vol. 18. p. 141 (1894); Hartert, "Vög. Pal. Fauna," part 3, p. 357 (1905).

Parus (*Periparus*) *ater britannicus*, Hellmayr, "Tierreich, Paridae," p. 77 (1902).

Periparus britannicus, Bianchi, *t.c.*, p. 245; Sharpe, "Handl. B.," vol. 4, p. 325 (1903).

Similar to *P. ater*, and with white spots on the median and greater coverts, but having the back olive-brown, instead of blue-grey; only the throat black; sides of the body very clear rusty-yellow: bill decidedly more slender.

Hab. British Islands.

Parus britannicus, when a series is compared, need never be confounded with true *P. ater*, for the difference in colour of the back is very perceptible. Freshly moulted birds in August and September are strongly olivaceous on the back, which in *P. ater* is clear blue-grey at that

time of year. On some specimens the white nape-patch shows a slight yellowish tinge, which may be a sign of a freshly moulted young bird.

The olive-brown colour of the back is always very pronounced from August and September to January; it becomes a little less obvious as the breeding-season approaches, and the colour of the back becomes slightly more grey as the plumage becomes worn, but it is never so blue as in the continental bird.

Parus vieiræ, n.sp. (Pl.)

There is in the British Museum a specimen, from Portugal, which appears to belong to an undescribed form of Coal-Tit, and for which I propose the name of *Parus vieirae*.

Similis *P. britannico*, sed regione uropygiali et corpore subtus pallide cinnamomeis distinguendus. Long. tot. 4·0, culm. 0·4, alae 2·2, cauda 1·6, tarsi 0·7.

Hab. Coimbra, Portugal (Dr. L. Vieira).

The dull cinnamon-rufous colour of the underparts which gives a rufous appearance to the bird is quite different from the fulvescent tint found in *P. ater* and *P. britannicus*. Dr. Sharpe tells me that he has shown the type to Dr. Bianchi and Dr. Hartert, and that they both confirm my idea of its distinctness.

Parus sardus.

Parus sardus, Kleinschmidt, *Orn. MB.*, vol. 11, p. 186 (1903).
Parus ater sardus, Hartert, "Vög. Pal. Fauna," part 3, p. 358 (1905).

This is said by Pastor Kleinschmidt to be easily recognisable from true *P. ater* by its bright rust-coloured sides. The English form, he adds, is not so bright in colour on the flanks, and has likewise a duller colouring

on the back. The British typical form is in every respect much duller and darker in colour than the Sardinian bird.

Hab. Sardinia.

Parus cypriotes.

Parus cypriotes, Dresser, *P.Z.S.*, 1867, p. 563, *id.* "Man. Pal. Birds," vol. 1, p. 165 (1902).

Parus ater cypriotes, Pražák, *MT. Orn. Ver. Wien*, vol. 18, p. 142 (1894); Hartert, "Vög. Pal. Fauna," part 3, p. 359 (1905).

Parus (*Periparus*) *ater cypriotes*, Hellm., *t.c.*, p. 77 (1903).

Periparus cypriotes, Bianchi, *t.c.*, p. 244; Sharpe, "Handl. B." vol. 4, p. 325 (1903).

With white spots on the wing-coverts as in *P. ater*, but with an olive-brown back, deeper in colour than in *P. britannicus;* lower neck black, as well as the throat: rest of under-surface of body cream-coloured, the sides and under tail-coverts washed with brownish.

Hab. Island of Cyprus.

Parus ledouci.

Parus ledouci, Malh., *Mém. Soc. H. N. Moselle*, p. 45 (1842): Dresser, "Man. Pal. Birds," vol. 1, p. 166 (1902).

Parus ater ledouci (Malh.), Pražák, *t.c.*, p. 20; Hartert, "Vög. Pal. Fauna," part 3, p. 360 (1905).

Parus (*Periparus*) *ater ledouci*, Hellmayr, "Tierreich, Paridae," p. 80 (1903).

Periparus ledouci, Bianchi, *t.c.*, p. 244; Sharpe, "Handl. B.," vol. 4, p. 324 (1903).

This species is confined to N.E. Africa, and is easily recognised by its yellow face and breast.

Its home is in Algeria and N. Tunis.

Parus atlas.

Parus atlas, Meade-Waldo, *Bull. Brit. Orn. Club*, 1901, p. 27; *id. Ibis*, 1903, p. 207, pl. 6; Dresser, "Man. Pal. Birds," vol. 1, app. p. 885 (1903).

time of year. On some specimens the white nape-patch shows a slight yellowish tinge, which may be a sign of a freshly moulted young bird.

The olive-brown colour of the back is always very pronounced from August and September to January; it becomes a little less obvious as the breeding-season approaches, and the colour of the back becomes slightly more grey as the plumage becomes worn, but it is never so blue as in the continental bird.

Parus vieiræ, n.sp. (Pl.)

There is in the British Museum a specimen, from Portugal, which appears to belong to an undescribed form of Coal-Tit, and for which I propose the name of *Parus vieirae*.

Similis *P. britannico*, sed regione uropygiali et corpore subtus pallide cinnamomeis distinguendus. Long. tot. 4·0, culm. 0·4, alae 2·2, cauda 1·6, tarsi 0·7.

Hab. Coimbra, Portugal (Dr. L. Vieira).

The dull cinnamon-rufous colour of the underparts which gives a rufous appearance to the bird is quite different from the fulvescent tint found in *P. ater* and *P. britannicus*. Dr. Sharpe tells me that he has shown the type to Dr. Bianchi and Dr. Hartert, and that they both confirm my idea of its distinctness.

Parus sardus.

Parus sardus, Kleinschmidt, *Orn. MB.*, vol. 11, p. 186 (1903).
Parus ater sardus, Hartert, "Vög. Pal. Fauna," part 3, p. 358 (1905).

This is said by Pastor Kleinschmidt to be easily recognisable from true *P. ater* by its bright rust-coloured sides. The English form, he adds, is not so bright in colour on the flanks, and has likewise a duller colouring

down on the sides of the neck. Median and greater wing-coverts with large white spots at the ends, which, on the central and inner greater coverts, are surrounded by a small edging of rusty yellow, and, as well as the primary-coverts, show an olive-brownish outer edge. Primaries whitish, the inner ones, as well as the secondaries, olive-greenish, the innermost secondaries more edged with brown. Tail feathers externally edged with olive-grey or brownish. The throat-patch often extends over part of the chest. Middle of under surface of body white; the sides of the body, abdomen, and under tail-coverts washed with fulvous rust-colour, the latter less strongly.

Hab. Persia and Southern Transcaspia.

Parus moltchanowi.

Parus moltchanowi, Menzbier, *Bull. Brit. Orn. Club*, vol. 13, p. 49, (1903), Dresser, "Man. Pal. Birds," vol. 1, app., p. 885 (1903).

Parus ater moltchanowi, Hartert, "Veg. Pal. Fauna," part 3, p. 360 (1905).

In this species the back is said to be of the same colour as in *P. ater*, but of a somewhat lighter grey, the under-surface of the body with scarcely any tint on the sides. Wings and tail as long as in *P. michalowskii*, the bill being much longer and slenderer (cf. Hartert, *l.c.* p. 360).

Hab. Mountains of Southern Crimea.

Parus derjugini.

Periparus ater var. derjugini, Sarudny and Loudon, *Orn. MB.*, vol. 11, p. 129 (1903).

Parus ater derjugini, Hartert, "Veg. Pal. Fauna," part 3, p. 360 (1905).

This form is said to differ from typical *P. ater*, in its longer and heavier bill, and there is a mixture of greyish-brown in the colour of the back.

It is found in the Tschorock District of the Caucasus.

Dr. Hartert says that it has a much longer bill than *P. moltchanowi*, and differs from that species, in that the back is not so pure ashy-grey, but is plainly olivaceous in tint, as in *P. britannicus*, while the sides of the body and the under tail-coverts are not so whitish, but clearly tinged with reddish-brown colour.

From *P. michalowskii* it differs in the much more slender bill and the colouring of the upper surface, which appears more greyish.

Parus michalowskii.

Parus michalowskii, Bogd., *Tr. Soc. Kazan Univ.*, vol. 8, no. 4, p. 87 (1879).

Parus ater michalowskii, Pražák, *MT. Orn. Ver. Wien*, vol. 18, p. 143 (1894); Hartert, "Vög. Pal. Fauna," part 3, p. 360 (1905).

Parus (Periparus) ater michalowskii, Hellmayr, "Tierreich, Paridae," p. 78.

Periparus michalowskii, Bianchi, *t.c.*, p. 244; Sharpe, "Handl. B.," vol. 4, p. 325 (1903).

This is one of the brown-backed section of Coal-Tits, and is very closely allied to *P. phaeonotus*, Blanford, with which Mr. Dresser has united it.

Mr. Hellmayr gives the characters as follows:—Very similar to *P. britannicus*, but with a somewhat brownish tone on the back; the rump without any olive-grey tint. The under surface, however is much lighter, and with much less of a bright wash on the sides. Bill decidedly stouter than in the allied forms, *P. ater* and *P. phaeonotus*.

The habitat is stated by Dr. Hartert to be the Southern Caucasus, in the river system of the Kura, Sekarsk to Kedabeg and Lenkoran.

Parus aemodius.

Parus aemodius, Hodgs., *J. A. S. Beng.*, vol. 13, p. 943 (1844).
Parus ater aemodius, Pražák. *t.c.*, p. 174 (1894); Hartert, "Vög. Pal. Fauna," part 3, p. 358 (1905).
Lophophanes aemodius, Oates, "Faun. Brit. Ind., Birds," vol. 1, p. 58 (1889).
Periparus aemodius, Bianchi, *t.c.*, p. 109 (1902); Sharpe, "Handl. B.," vol. 4, p. 326 (1903).
Parus (*Periparus*) *ater aemodius*, Hellmayr, "Tierreich, Paridae," p. 79 (1902).

This species is distinguished by Mr. Hellmayr from the other races of *P. ater*, on account of its ochre-yellow under surface. I should have called the breast of *P. aemodius* pale 'cinnamon-buff,' but I find that it agrees most nearly with the 'ochraceous-buff' of Ridgway's 'Nomenclature of colours.' In any case, this rufescent tint of the under surface generally serves to distinguish *P. aemodius* and *P. rufipectus* from *P. ater* and its allies, though *P. pekinensis* often shows a rufous tint on the under surface. In *P. ater* and the other forms there is sometimes a tint of pale buff or cinnamon, but their general aspect below is greyish white.

1 PARUS VIRIDIS
2 . . BRITANNICUS

Manchester Memoirs, Vol. l. (1906), *No.* **14.**

XIV. The species of Ctenopteryx, a genus of Dibranchiate Cephalopoda.

By Dr. J. H. ASHWORTH and Dr. W. E. HOYLE.

Read May 8th, 1906. Received for publication July 24th, 1906.

The genus under discussion was created in 1890 by Dr. Appellöf for a small Cephalopod from Messina, only 15 mm. in length, and characterised as follows:—

Ctenopteryx Appellöf ('90, p. 3).

The fins consist of a series of muscular threads, united down to the base by a very thin, transparent membrane, so that each fin has a comb-like appearance. The mantle connective consists of a tract of cartilage on either side of the base of the funnel, wider behind, and with a very narrow groove down its middle, corresponding to which is a cartilaginous ridge on the inner side of the mantle. Only two pairs of siphonal adductors are present, of which the upper pair are visible from without; an external muscle from the funnel to the head, as seen in *Ommastrephes*, is absent. The ocular opening is drawn out forwards into a pointed sinus, so that it assumes a pear-shaped appearance. There is no clearly defined funnel groove; the funnel has a valve.

The following forms have been referred to this genus by various authors:—

Ctenopteryx fimbriatus, Appellöf (type).
Ctenopteryx cyprinoides, Joubin.
Calliteuthis nevroptera, Jatta.
Sepioteuthis sicula, Rüppell.

We propose to discuss these identifications seriatim.

August 14th, 1906.

Ctenopteryx cyprinoides, Joubin ('94, :01).

This species was based on three specimens taken from the stomach of a dolphin, which was captured by the Prince of Monaco's yacht "Princesse Alice," off the island of Corsica. Pfeffer (:00) has expressed the opinion that these two names belong to one and the same species, though without giving any reasons for his belief. Jatta ('03), however, held the view that they were probably distinct, judging only from the published descriptions.

A short time ago one of us was fortunate enough to procure a well-preserved specimen* from Messina, much larger than that described by Appellöf, and comparable in size with the Monaco examples, as the following table of dimensions will show:—

DIMENSIONS IN MILLIMETRES.

	Our specimen.		Monaco specimen.
Length, total	129		—
Length, total, excluding tentacles ...	100		112
End of body to mantle margin (dorsal)	54		51
End of body to eye	59		—
Breadth of body	21		32†
Breadth of head	22		23
Eye to edge of umbrella (laterally)...	12		—
Length of fin	R. 48, L 51		—
Breadth of fin	12		22
Breadth across fins	38		—
	Right.	Left.	
Length of first arm	25	25	33
Length of second arm	29	27	36
Length of third arm	29	20	31
Length of fourth arm	28	28	29
Length of tentacle	57	68	150

* I would here express my thanks to the Carnegie Trust for the Universities of Scotland for a grant which enabled me to purchase this specimen.—J. H. A.

† Measured at the opening of the mantle.

In order to settle the question of the identity of this species with that of Appellöf, we applied to Dr. Richard, director of the Musée océanographique at Monaco, for permission to examine Joubin's types, and with a generous courtesy, which we most gratefully acknowledge, he was good enough to entrust them to us for comparison. The following is the result at which we have arrived.

The characters upon which Joubin relies for the distinction of the two species are :—

C. fimbriatus.	*C. cyprinoides.*
1. The pectinate fin stops about one quarter of the length of the mantle behind its anterior margin.	1. The pectinate fin extends forward as far as the margin of the mantle.
2. The fin has 24 teeth.	2. The fin has 25 or 26 teeth, and possibly one more, which cannot be made out with certainty owing to the unsatisfactory condition of the specimens.
3. The mantle projects backwards into a notch left between the two fins.	3. The fins are continuous round the hinder end of the mantle so that no notch is formed.
4. The tentacles at most reach a length equal to that of the mantle.	4. The tentacles are at least three times as long as the mantle.
5. The head is sunk into the mantle, and its breadth appears to be about equal to the diameter of the mantle opening.	5. The head is less deeply sunk into the mantle, and its breadth is less than the opening of the latter.

In regard to these several points our example presents the following appearances:—

1. The pectinated fin extends practically the whole length of the mantle. The ceasing of the fin behind the anterior margin of the mantle in Appellöf's type we regard as due to its immaturity: a similar condition is seen in several other young examples which have come into our hands.

2. The pectinated fin has 23 teeth on the left and 24 on the right side. On the specimen from Monaco which appeared to be best preserved we only succeeded in counting 24 teeth on each side, but it is by no means easy to be sure of the number owing to the unsatisfactory state of preservation.

3. The fin is very narrow posteriorly, but is continuous round the hinder end of the mantle. Here, again, we attribute the deficiency in Appellöf's type to immaturity. In seven young examples examined by one of us, ranging from about 4 mm. to 9 mm. in length, the fins are clearly separate from each other, though in one or two cases there is only a short interval between them.

4. The tentacles are somewhat longer than the mantle: their excessive length in the types of *C. cyprinoides* we attribute to the maceration which they have undergone. This elongation of the tentacles is of very frequent occurrence in cephalopods taken from the stomachs of cetaceans.

5. This character we do not regard as of any systematic importance. The extent to which the head is drawn into the mantle varies greatly in different examples of the same species and depends on the state of contraction of the retractor muscles at the time of death. The diameter of the head in relation to the size of the opening of the mantle is also dependent on the state of contraction of the latter.

Taking these facts into consideration we cannot see that there is any ground for regarding the Monaco specimens as being specifically distinct from the one from Messina, examined by us, and this again differs from Appellof's type only in characters which are dependent on difference in age. We, therefore, regard *C. cyprinoides* as a synonym of *C. fimbriatus*.

It must be remembered, however, that the specimens examined by Joubin had undergone severe maceration by the gastric juices of the dolphin from whose stomach they were taken and that he only had for comparison with them the figures and description of a much smaller example. Hence he was led to attach more importance to the apparent differences than they deserved

Calliteuthis nevroptera, Jatta ('96, p. 118, pl. 31, figs 1—10).

Unfortunately the type specimen of Dr. Jatta's species appears to have been dissected and the parts not preserved The principal differences between his published description and our specimen are as follows:—

The fin in Jatta's specimen extends along two-thirds of the mantle, while in ours it is present along practically the whole length of the mantle. This difference is probably accounted for by the fact that ours is larger and more mature The tentacular arms are scarcely clubbed in our specimen, the dilatation being of the slightest. The margin of the ocular opening is not perfectly round, as in Jatta's example, but bears a notch at its antero-ventral border, this is exactly the same on both right and left sides. The transparent elliptical disc behind the eye, referred to by Jatta, is not pronounced in our specimen. There is on the left side, behind the eye, a paler area, somewhat reniform in shape, its concave face turned to the

eye, which was probably more transparent in life, and crossed by three round pigment spots. On the right side this area is larger, not so regular in shape, and less clearly defined. On the lower side of the head are two elongate "olfactory organs," apparently not quite so triangular as those of Jatta's specimen. The buccal membrane is well developed. The characters of the body in general, the front edge of the mantle, the nuchal cartilage, and the arms seem to agree with those of Jatta's specimen.

The differences do not appear to us of generic value, except as regards the presence or absence of a sinus in the anterior margin of the ocular opening. A difference in this respect has usually been regarded as marking a generic distinction, but we do not think it of sufficient importance to outweigh the many points of resemblance, especially as it seems to vary in extent in different examples (compare Appellöf's description and figure with the description of our specimen above), and may prove to be dependent on the age of the specimen.

Jatta himself subsequently (:03) came to the conclusion that his species should be referred to the genus *Ctenopteryx*.

It is more difficult to form an opinion as to the specific identity of these two forms. Dr. Pfeffer (:00) has united them; for ourselves we would merely say that we have a strong suspicion that they will ultimately prove to be the same, but pending further evidence it is advisable to retain Dr. Jatta's name.

Sepioteuthis sicula, Rüppell: Verany ('51, p. 75, pl. 27).

Dr. Pfeffer has regarded this form as identical with Appellöf's species, but the characteristic features of *Ctenopteryx* are not indicated by Verany in his drawing, though a phrase in the description of the colour might be

interpreted as referring to the remarkable pectinate form of the fins.* The dimensions given ("trois décimètres"), unless a misprint, indicate a much larger form than any *Ctenopteryx* hitherto recorded. We are, on the whole, disposed to agree with Jatta in regarding Ruppell's species as a doubtful form, the fate of which can only be decided when further material is available.

SUMMARY.

We conclude that *Ctenopteryx cyprinoides*, Joubin ('94), is to be regarded as a synonym of *C. fimbriatus*, Appellof ('90). *C.* (*Calliteuthis*) *neroptera*, Jatta ('96), is also probably to be referred to *C. fimbriatus*, but pending further evidence Jatta's specific name is retained. *Sepioteuthis sicula*, Rüppell (Verany '51), which has also been considered to be identical with Appellof's species, is a doubtful form, the position of which cannot at present be determined with certainty.

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* "Après la mort le corps de ce céphalopode prend un teint blanchâtre par l'action de l'alcool, les fibres musculaires horizontales des nageoires deviennent opaques et les nageoires semblent comme rayées horizontalement de blanc.

"Les peu d'individus de cette espece, qui ont été pris n'ont jamais dépassé trois décimetres de longueur, non compris les bras tentaculaires" (p. 76).

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Mr. H. MORRIS AIREY, M.Sc., read a paper entitled "**On the Variation of the Electrical Resistance of Osmium with Temperature.**" The range over which the experiments were conducted extended from the temperature of liquid air up to dull red heat. The results show that the behaviour of osmium, like that of the ordinary metals, can be represented by a parabolic expression.

December 12th, 1905.

The Ordinary Meeting fixed for this date was not called, but a Conversazione was held in the Society's House on the invitation of the President and Council. 260 cards of invitation were issued to members and other persons not connected with the Society, and a full descriptive catalogue of the very interesting exhibits brought together on the occasion was printed and distributed to those present.

Ordinary Meeting, January 16th, 1906.

The President, SIR WILLIAM H. BAILEY, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Mr. H. STANSFIELD, B.Sc., described the behaviour of liquid films formed from a solution of saponin in water. Although saponin films have very little mobility, they are capable of becoming extremely thin. The limiting thickness of a black saponin film is comparable with that of the thinnest soap film. In the process of thinning, the saponin films exhibit a grey stage; and there are two characteristic abrupt changes in thickness, the first from the white of the first order to the grey, and the second from the grey to the black.

Mr. THOMAS THORP, F.R.A.S., exhibited a lantern slide photograph of the total solar eclipse, taken at Burgos, in Spain, on August 30th, 1905. The inner corona and several streamers, extending nearly a degree from the sun, were shown very clearly, as also a number of prominences.

Mr. C. G. HEWITT, B.Sc., read a paper entitled "**Note on the Buccal Pits of Peripatus.**"

In the discussion which followed Professor S. J. HICKSON, F.R.S., pointed out that the peculiar position which this small creature occupied as a connecting link between the worms and insects, and the probability that it would soon become extinct, gave a special importance to any investigation which resulted in additional knowledge of the anatomy of Peripatus.

General Meeting, October 17th, 1905.

The President, Sir WILLIAM H. BAILEY, in the Chair.

Mr. C. GORDON HEWITT, B.Sc., Assistant Lecturer and Demonstrator in Zoology in the Manchester University, was elected an ordinary member of the Society.

Ordinary Meeting, October 17th, 1905.

The President, Sir WILLIAM H. BAILEY, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Mr. T. THORP, F.R.A.S., made a short communication on the "shadow bands" seen only during a total eclipse, which were well observed at Burgos, in Old Castile, on August 30th, 1905.

The PRESIDENT then delivered his **Inaugural Address**.

The address is published in full in the "Memoirs."

General Meeting, October 31st, 1905.

Professor W. BOYD DAWKINS, D.Sc., F.R.S., Vice-President, in the Chair.

Mr. HERBERT J. WOODALL, Associate of the Royal College of Science of London, *Market Place, Stockport*; Miss MARY MCNICOL, B.Sc., Research Scholar in the Manchester University; Miss ETHEL G. WILLIS, B.Sc., Science Mistress, Manchester High School for Girls; Miss EDITH MARY SAXELBY, B.Sc., Research Scholar in the Manchester University, were elected ordinary members of the Society.

Ordinary Meeting, October 31st, 1905.

Professor W. BOYD DAWKINS, D.Sc., F.R.S., Vice-President, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Mr. F. J. FARADAY, F.L.S., drew attention to a paper entitled "On a Biological Aspect of Cancer," read by him in 1899, and printed in volume 43 of the Society's "Memoirs."

Several of the conclusions recently arrived at by the Cancer Research Committee were therein foreshadowed, *e.g.*, that cancer is not a microbic disease, but is due to an arrest of development and differentiation among the somatic cells, growth being restricted to mere gemmation. The cure consequently resolved itself into finding out the nature of the change in nutrition, nerve-stimulus, or environment, which might be the cause of this change in the normal life-history of the somatic-cells. In illustration deficient oxygenization of the blood might be hypothetically regarded from analogy as a possible cause.

Dr. MARIE C. STOPES gave an account of some recent researches into the nutrition of the egg cell in certain plants. The special group of plants on which the author worked was

that including the pine trees, Ginkgo, and the Cycads, viz. :— the Gymnosperms. Though the egg cells in this group are in many ways different from those of the flowering plants, the results have some bearing on the question of nutrition of egg cells in general, as well as some points of general technique.

The egg cells in all the plants under consideration are surrounded by a well-marked layer of cells, called the "jacket cells." In the past, it has been stated by Arnoldi, Ikeno, Coulter, and Chamberlain, and others that the nuclei of these cells enter the egg cell either bodily or in part, and so provide nourishment for the growing egg. The "proteid vacuoles" in the egg of *Pinus* and the proteid granules in *Ginkgo* and *Cycas* had been traced directly or indirectly to these nuclei.

The author shows that the entry of these nuclei does not usually take place, and that the structure of the wall of the egg, in which a fine membrane closes all pits, makes it impossible under normal conditions; and that it is not necessary to look only to the nuclei of these jacket cells for the supply of proteid, as the whole surrounding tissue in *Ginkgo* and *Cycas* is packed with absolutely similar proteid grains to those in the egg. It was also found, during research in an Alpine laboratory, where the author examined living material every three hours during the day, that starch grains occur *in the egg cell* as well as in the surrounding tissue. The author lays stress on the fact that carbohydrate (*i.e.*, starch) always travels in soluble form as sugar; that proteid also travels in some soluble, simpler form; and that it is unnatural to expect a sudden change in the mode of entry to the egg, as would be the case if the nuclei of surrounding cells entered it as such.

The jacket cells act as a secreting layer and dissolve the food stored in the endosperm, which then passes into the egg in the normal way in solution.

In conclusion, the author states that the past unnatural views have probably resulted from too close attention to material treated by elaborate technical methods, and that her work on treated material was always checked by work on living material.

Much of the work was done in conjunction with Prof. Fujii, of Tokio, with whom the author is publishing a joint paper on the subject in Germany.

Dr. HERBERT RAMSDEN exhibited and described a model, devised by himself, to illustrate the propagation of sound waves.

It consists of a series of magnetised needles, suspended vertically so as to vibrate in the same plane with their like poles downwards, and is designed to show (since the needles were constructed and regulated to have equal times of oscillation) most of the phenomena of the longitudinal transmission of waves.

General Meeting, November 14th, 1905.

The President, Sir WILLIAM H. BAILEY, in the Chair.

Mr. GEORGE C. SIMPSON, M.Sc., Lecturer in Meteorology in the University of Manchester, of *Dalton Hall, Victoria Park*; Mr. CHARLES H. BURGESS, M.Sc., Demonstrator of Chemistry in the University of Manchester, *Shaftesbury House, Cheadle Hulme*; Mr. ALFRED HOLT. M.A., Research Fellow of the University of Manchester; and Mr. WILLIAM EDWIN GRIMSHAW, B.A., Physics Master, Manchester Grammar School, *46, Broadway Street, Oldham*, were elected Ordinary Members of the Society.

Ordinary Meeting, November 14th, 1905.

The President, Sir WILLIAM H. BAILEY, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

The following were among the recent accessions to the Society's Library:—"*The Collected Mathematical Works*" of G. W. Hill, vol. I (4to., Washington, 1905), presented by the author through the Carnegie Institution; "*The Financial History*

of Baltimore," by J. H. Hollander (8vo., Baltimore, 1899), presented by the Johns Hopkins University; "*Le Opere*" di Galileo Galilei, vol. 16 (4to., Firenze, 1905), presented by the Italian Embassy at London; "*Catalogue of Greek Coins in the Hunterian Collection*," vol. 3, by G. Macdonald (4to., Glasgow, 1905), presented by the Trustees of the Hunterian Coin Catalogue Fund; "*Mexico, its Social Evolution*," 3 vols. (Fol., Mexico, 1900, 1902), presented by the Minister of Commerce and Industry of the Mexican Government.

The thanks of the members were also voted to Mr. Alfred Brothers, F.R.A.S., for his presentation to the Society of a portrait of the late Rev. William Gaskell, who was a Vice-President during the years 1869-75 and 1882-83.

Professor H. B. DIXON, M.A., F.R.S., who was present at the meetings of the British Association in South Africa, exhibited a number of slides, made from photographs taken by him of some of the places of interest he visited, and of the various types of natives.

Mr. C. L. BARNES, M.A., read some extracts from the Classical writers, which showed in how little esteem seaweed was held by the ancients, it being regarded by them as the most useless of things. He then pointed out, by enumerating some of the uses to which the seaweed is now put, that the moderns had effectually removed this reproach from it.

Mr. C. H. BURGESS, M.Sc., read a paper entitled "**Some Convection Effects in a Heated Tube.**" The author performed the experiment described in his paper.

Ordinary Meeting, November 28th, 1905.

The President, Sir WILLIAM H. BAILEY, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Mr. H. MORRIS-AIREY, M.Sc., read a paper entitled "**On the Variation of the Electrical Resistance of Osmium with Temperature.**" The range over which the experiments were conducted extended from the temperature of liquid air up to dull red heat. The results show that the behaviour of osmium, like that of the ordinary metals, can be represented by a parabolic expression.

December 12th, 1905.

The Ordinary Meeting fixed for this date was not called, but a Conversazione was held in the Society's House on the invitation of the President and Council. 260 cards of invitation were issued to members and other persons not connected with the Society, and a full descriptive catalogue of the very interesting exhibits brought together on the occasion was printed and distributed to those present.

Ordinary Meeting, January 16th, 1906.

The President, Sir WILLIAM H. BAILEY, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Mr. H. STANSFIELD, B.Sc., described the behaviour of liquid films formed from a solution of saponin in water. Although saponin films have very little mobility, they are capable of becoming extremely thin. The limiting thickness of a black saponin film is comparable with that of the thinnest soap film. In the process of thinning, the saponin films exhibit a grey stage; and there are two characteristic abrupt changes in thickness, the first from the white of the first order to the grey, and the second from the grey to the black.

In the apparatus exhibited platinum wires were introduced into the film in order that, after it had become black, it could be conveniently thickened again so as to show the grey stage by applying an electromotive force of a few volts. The motion of material that is observed in the film is in the same direction as the electromotive force.

Mr. JOHN ALLAN read a paper, communicated by Professor E. Knecht, Ph.D., F.C.S., entitled, "**On Battack Printing in Java, with Notes on the Malay Kris, and the Bornean Sumpitan and Upas Poison.**"

Specimens of the objects described were exhibited.

Mr. J. W. JENKINSON, M.A., of Exeter College, Oxford, read a paper, communicated by Dr. F. W. GAMBLE, entitled "**Remarks on the Germinal Layers of Vertebrates and on the Significance of Germinal Layers in general.**"

General Meeting, January 30th, 1906.

The President, Sir WILLIAM H. BAILEY, in the Chair.

Mr. STANLEY DUNKERLEY, D.Sc., Professor of Engineering in the University of Manchester, was elected an ordinary member of the Society.

Ordinary Meeting, January 30th, 1906.

The President, Sir WILLIAM H. BAILEY, in the Chair.

The thanks of the members were voted to the donors of the books upon the table. The following were among the recent accessions to the Society's Library:—"*Bibliographical Index of North American Fungi*," by W. G. Farlow, vol. 1, pt. 1. (8vo.,

Washington, 1905), presented by the Carnegie Institution at Washington; "*Mexican and Central American Antiquities*," by E. Seller and others (8vo., Washington, 1904), presented by the Bureau of American Ethnology; "*A Monograph of the British Desmidiaceæ*," vol. 2, by W. West and G. S. West (8vo., London, 1905), "*The British Freshwater Rhizopoda and Heliozoa*," vol. 1, by J. Cash and J. Hopkinson (8vo., London, 1905), purchased from the Ray Society; "*Les Quantités élémentaires d'Électricité: Ions, Électrons, Corpuscules*," par H. Abraham et P. Langevin, fasc. 1, 2 (8vo., Paris, 1905), presented by the Société Française de Physique; "*Déscription géologique de l'Île d'Ambon . . .*", par R. D. M. Verbeek (8vo., Batavia, 1905), "*Atlas*" (fol., Batavia, 1905), presented by the Dutch Colonial Department.

Mr. C. L. BARNES, M.A., shewed a group of stereoscopic charts of the stars, issued with Mr. T. E. Heath's "Our Stellar Universe." The charts are reduced from maps on which are represented all the stars whose parallaxes have been measured, or are conjecturable with some approach to accuracy. Since the actual parallaxes are seldom more than small fractions of a second of arc, it has been found necessary to magnify them about 19,000 times for purposes of convenience. There are twenty-six maps in all, two of which shew the polar regions on a polar projection, eight the equatorial regions on Mercator's projection, and the remainder the intermediate declinations and latitudes, north and south, on a conical projection. The object aimed at is to shew the solidity of space, and to represent the stars, not as on the surface of a sphere, but at different distances, such as they actually occupy.

At this point the Chair was taken by Mr. FRANCIS NICHOLSON, F.Z.S.

Mr. R. L. TAYLOR, F.C.S., F.I.C., read the following paper entitled, **"On the Origin of the Salt in the Sea."**

This paper is a contribution to the controversy on this subject, which was started more than thirty years ago by Dr. Sterry Hunt.

The most recent and reliable estimate of the total amount of sea-water which exists gives it at 340 millions of cubic miles. A cubic mile of sea-water contains, at the rate of about 4·3 ounces per gallon, 110 million tons of common salt, and, in addition, 3·6 of potassium chloride, 12 of magnesium chloride, 8 of magnesium sulphate, and 5·5 of calcium sulphate—all in millions of tons.

Sterry Hunt's hypothesis* was, shortly, as follows:—When the earth was in an intensely heated gaseous condition, all the elements which compose it would be in the free state, and would only unite gradually as the temperature fell. When the temperature reached 1200°C. the crust at any rate would have become solid, and would inevitably consist mainly of silicates such as we are familiar with as forming the principal constituents of eruptive and igneous rocks. Practically all the chlorine which now exists as chlorides would then be in the atmosphere in the form of hydrochloric acid; the carbon which exists now as coal, as well as that in the carbonates of lime and magnesium which now form such an important part of the earth's crust, would also be in the atmosphere as carbonic acid gas, and in all probability the sulphur which now exists in the various natural sulphates and sulphides would be in the atmosphere as well, either in the form of sulphur dioxide or trioxide. The whole of the water would also be in the atmosphere in the form of vapour, so that this primeval atmosphere must have exerted an enormous pressure—possibly 300 or 400 times its present pressure. As the temperature fell, the water vapour would at last condense to liquid water. The condensation would begin at 370°C (the critical temperature of water), and the water would immediately dissolve in it some, at any rate, of the acid gases from the atmosphere. The primitive ocean would therefore be a highly heated dilute solution of hydrochloric and sulphuric acids. The chemist will readily understand how rapidly this would attack many of the natural silicates, the acids finally becoming completely neutralised, and the ocean becoming

**Chemical News*, vol. 15, p. 314.

a solution of chlorides and sulphates, in which calcium, and perhaps magnesium, would probably preponderate as bases. Much silica would also enter into solution, to be afterwards deposited in the crystalline form. Insoluble or slightly soluble sulphates such as those of barium and calcium, would also sooner or later separate out. The atmosphere would still contain a large amount of carbon dioxide, and the gradual decomposition of the exposed parts of the earth's crust by the moisture and carbon dioxide would result in the dissolving out of the alkalies as carbonates, which would be carried into the sea. The immense amount of calcium which existed in the primitive ocean has since been removed by the agency of organisms of various kinds, and now exists as calcium carbonate, in the form of limestone chalk, coral, &c. The place of the calcium in the water would thus gradually be taken by the sodium carried down by the rivers.

David Forbes objected strongly to Sterry Hunt's views, and an animated and finally somewhat acrid controversy took place. Forbes contended that, in the cooling globe, the chlorine would unite with sodium in preference to hydrogen, and thus common salt would be deposited all over the globe long before the water became liquid. Forbes said that there would be a layer of salt 10 feet thick all over the globe, which would be immediately dissolved when the water condensed, so that, according to him, the sea would be salt from the very beginning. (Forbes' calculation was lamentably wrong; Joly has since calculated that there is enough salt in the sea to form a layer 112 feet thick over the whole globe.)

Probably no modern chemist would accept Forbes' view. The temperature at which common salt would solidify would be quite high enough for silica and silicates, in conjunction with water vapour, of which there would be an immense amount, to decompose it, and this of course would result in the liberation of hydrochloric acid. Further, authorities are pretty well agreed that there is nearly as much potassium as there is sodium in the earth's crust, and as the heat of combination of chlorine with

potassium is *higher* than with sodium, if the chlorine united with an alkali metal at all it would be with potassium preferably, with the result that potassium chloride would have been the principal constituent of sea-water.

Hunt's hypothesis gives a reasonable explanation of the facts as they exist at the present time. It accounts, on the one hand, for the immense quantities of common salt and of calcium and magnesium carbonates which are known to exist, and on the other hand it explains the tremendous amount of silica which is found in the free state as sand, sandstone, and other forms. These, basic and acid bodies, are the complements of each other, and they formerly existed in combination as more or less complicated silicates.

Under any hypothesis as to the condition of the primeval earth and ocean, one of the most difficult things to explain is the fact that while the metals sodium and potassium probably exist in approximately equal quantities in the earth's crust, the salts of the former predominate so greatly in the waters of the ocean at the present time. There are several facts, however, which go a long way towards explaining this anomaly. First, there is the formation of marine deposits containing potash, of which the mineral Glauconite is a good example. This mineral is a silicate of iron and potassium, and appears to be a product of the decomposition of certain marine organisms. It is forming in various places at the present time, and is widely distributed amongst the sedimentary rocks. There is no doubt that the formation of substances such as Glauconite will account for the abstraction of a considerable amount of potash from the waters of the ocean.

Then there is another fact (which Mendelejeff appeared to think of great importance) namely, that soil of any kind, especially when mixed with vegetable remains, *retains* compounds of potassium much more readily than those of sodium, and this probably will account for some more of the deficiency of potassium salts in the ocean. There is also another consideration to which I myself should attach considerable importance, and

that is the fact that silicates in which soda is the principal alkali are, on the whole, more readily decomposed, both by hydrochloric acid and by the ordinary weathering processes of the atmosphere, than the corresponding silicates containing potassium. I have a list of some of the commoner silicates containing sodium either as the principal or one of the principal alkalies, and including such minerals as Albite, Natrolite, Nephelite, Sodalite, Pectolite, &c., and out of 15 such minerals no less than eight are decomposed by dilute hydrochloric acid,—one or two only with difficulty, but most of them easily. The commoner silicates containing potassium do not appear to be decomposed anything like so easily. I should contend, therefore, that at the first attack of the dilute acid of the primeval ocean upon the silicates of the crust, those containing sodium would yield more readily than those containing potassium, so that there would be a greater amount of sodium chloride than of potassium chloride in the sea from the very first. There is strong evidence that such a preferential action has taken place in the well-established fact that, whereas the average amount of soda in the igneous and eruptive rocks is higher than that of the potash in the proportion of about four to three, in the sedimentary rocks, which have been derived from the igneous and eruptive rocks, the proportion of soda to potash is only about three to five. Plainly the soda has been much more completely dissolved out than the potash.

The paper gave rise to an interesting discussion, in which several members took part.

Ordinary Meeting, February 13th, 1906.

The President, Sir WILLIAM H. BAILEY, in the Chair.

Dr. R. S. HUTTON and Mr. C. S. ALLOTT, M.Inst.C.E., were nominated auditors of the Society's accounts for the session 1905-1906.

The thanks of the members were voted to the donors of the books upon the table.

Mr. CHARLES BAILEY, M.Sc., F.L.S., presented to the Society's Library a quarto MS. volume, found at a bookseller's, containing records of the minutes of the meetings of the Manchester Botanists' Association. The first entry is dated January 13th, 1861, and the last April, 1889.

The thanks of the meeting were voted to Mr. Bailey for his interesting gift.

Mr. H. SIDEBOTTOM read a paper entitled, "**Report on the Recent Foraminifera from the Coast of the Island of Delos." Part III. Lageninae.** Drawings of the most interesting forms obtained were exhibited and described.

General Meeting, February 27th, 1906.

Professor HORACE LAMB, LL.D., D.Sc., F.R.S., in the Chair.

Mr. JOSEPH BURTON, A.R.Coll.Sc. Dublin, of the Tile Works, Clifton Junction, was elected an ordinary member of the Society.

Ordinary Meeting, February 27th, 1906.

Professor HORACE LAMB, LL.D., D.Sc., F.R.S., in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Mr. C. E. STROMEYER, M.Inst.C.E., gave notice that on March 27th he would move the following resolution :—

General Meeting, March 27th, 1906.

The President, Sir WILLIAM H. BAILEY, in the Chair.

The following resolution, proposed by Mr. C. E. STROMEYER, M.Inst.C.E., and seconded by Mr. E. F. LANGE, was carried :—

"That before every Annual Meeting the out-going Council "shall prepare ballot papers containing the names of "Members whom they have nominated for election into "the Council and for the various Offices. These ballot "papers to contain the names of two new Members, "due regard being taken that the various Literary and

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Special Meeting, March 20th, 1906.

The President, Sir WILLIAM H. BAILEY, in the Chair.

The Wilde Lecture, on **"Total Solar Eclipses,"** was delivered by H. H. TURNER, Esq., D.Sc., F.R.S., Professor of Astronomy in the University of Oxford.

Ordinary Meeting, March 27th, 1906.

The President, SIR WILLIAM H. BAILEY, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Mr. C. E. STROMEYER, M.Inst.C.E., read a paper entitled, **"On Recent Mysterious Fractures of Steel Plates."**

The author briefly referred to the well-known failures of steel plates of the boilers of the Imperial Russian Yacht "Livadia," and to Mr. Maginnis's experiences with two brittle marine boilers,

Ordinary Meeting, March 13th, 1906.

Professor W. BOYD DAWKINS, D.Sc., F.R.S., in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Mr. FRANCIS NICHOLSON, F.Z.S., exhibited a specimen of the Pine Marten, *martes abietum*, which was captured in Queen's County, Ireland, last April. He mentioned that Martens, whilst they do not appear to be very suspicious of traps as some wild animals are, yet range so far and in such varied directions, seldom following the same run, that they are only taken by accident. The specimen exhibited was so caught, in a trap set for rabbits, and singularly enough by the nail only of one of its toes. As regards its distribution, the Marten has met with the same fate in Ireland as in England and Scotland. At one time it was common throughout the island, but with the advance of civilisation,—railways, cultivation and the deforesting of woods, it has been driven to the wildest parts of the country, but not necessarily to the North and West.

Mr. Nicholson afterwards presented the specimen to the Manchester Museum at the University.

Mr. C. GORDON HEWITT, B.Sc., read a paper entitled "**The Cytological Aspect of Parthenogenesis in Insects.**"

Mr. W. THOMSON, F.R.S.E., F.I.C., read a paper entitled "**Notes on Arsenic and on its Estimation in Minute Quantities.**"

General Meeting, March 27th, 1906.

The President, Sir WILLIAM H. BAILEY, in the Chair.

The following resolution, proposed by Mr. C. E. STROMEYER, M.Inst.C.E., and seconded by Mr. E. F. LANGE, was carried:—

"That before every Annual Meeting the out-going Council "shall prepare ballot papers containing the names of "Members whom they have nominated for election into "the Council and for the various Offices. These ballot "papers to contain the names of two new Members, "due regard being taken that the various Literary and "Philosophical interests of Manchester are adequately "represented on the ballot papers. The President and "Vice-Presidents shall, as far as possible, be selected "for nomination from the out-going Members of "Council.

"A notice to be printed on the ballot papers "pointing out that, while other names may be substituted "for those nominated by the Council, the total number "voted for must not exceed the number of vacancies, "otherwise, the ballot paper will be considered void."

Ordinary Meeting, March 27th, 1906.

The President, SIR WILLIAM H. BAILEY, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Mr. C. E. STROMEYER, M.Inst.C.E., read a paper entitled, **"On Recent Mysterious Fractures of Steel Plates."**

The author briefly referred to the well-known failures of steel plates of the boilers of the Imperial Russian Yacht "Livadia," and to Mr. Maginnis's experiences with two brittle marine boilers,

Ordinary Meeting, March 13th, 1906.

Professor W. BOYD DAWKINS, D.Sc., F.R.S., in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Mr. FRANCIS NICHOLSON, F.Z.S., exhibited a specimen of the Pine Marten, *martes abietum*, which was captured in Queen's County, Ireland, last April. He mentioned that

Manchester Museum at the University.

Mr. C. GORDON HEWITT, B.Sc., read a paper entitled "**The Cytological Aspect of Parthenogenesis in Insects.**"

Mr. W. THOMSON, F.R.S.E., F.I.C., read a paper entitled "**Notes on Arsenic and on its Estimation in Minute Quantities.**"

Ordinary Meeting, April 10th, 1906.

Mr. FRANCIS NICHOLSON, F.Z.S., Vice-President, in the Chair.

The thanks of the members were voted to the donors of the books upon the table. The following were among the recent accessions to the Society's Library:—"*Report of the Committee upon Mechanical Coal-cutting*," pt. 2 (8vo., Newcastle-upon-Tyne, 1905), presented by the North of England Institute of Mining and Mechanical Engineers; "*Haida Texts and Myths, Skidegate Dialect*," recorded by J. R. Swanton (8vo., Washington, 1905), presented by the Bureau of American Ethnology; "*Codex Diplomaticus Lusatiae Superioris* iii. . . .", Hft. 1, von. Dr. R. Jecht, (8vo., Görlitz, 1905), "*Die mittelalterliche Baukunst Bautzens*," von F. Rauda (4to., Görlitz, 1905), presented by the Oberlausitzische Gesellschaft der Wissenschaften zu Görlitz; "*Reports of the Commission . . . for the Investigation of Mediterranean Fever*," pts. 1-4 (8vo., London, 1905-06), presented by the Royal Society of London; "*On the Flinders Petrie Papyri*," by J. P. Mahaffy and J. G. Smyly (Cunningham Memoirs, No. 11), (4to., Dublin, 1905), presented by the Royal Irish Academy; "*Idea dell' Universo ovvero Interpretazione della Natura e sue Conseguenze teoriche e pratiche*," di G. C. Paoli, vol. 1 (8vo., Milano, 1906), presented by the author; "*The Collected Mathematical Works of G. W. Hill*," vol. 2, (4to., Washington, 1906), presented by the author through the Carnegie Institution.

Mr. CHARLES OLDHAM read a paper entitled "**Notes on a Captive Mole**," by LIONEL E. ADAMS, B.A.

At this point the Chair was occupied by Dr. W. E. HOYLE, F.R.S.E., while Mr. FRANCIS NICHOLSON, F.Z.S., read a paper entitled "**Notes on the Palæarctic Species of Coal-Tits**."

and then dealt with recent cases which have come under his notice. He has collected samples of plates of exploded boilers from America, Austria, and Russia, and plates which have shewn themselves brittle in the workshop. In one case the brittleness is clearly due to an excess of phosphorus, but in all the other cases there is no discernible cause, and the author suggested that certain qualities of steel have the property of slowly deteriorating. Several of the fractured plates were exhibited, and sections were prepared shewing the microscopic structure of the different materials. The chemical compositions and the mechanical tests were also given.

At the end of the paper the author mentioned that he was about to carry out experiments on 20 samples of steel which he had collected, and asked for suggestions as to supplementary tests which would discriminate between reliable and treacherous steels.

An interesting discussion followed the reading of the paper, in which the President, Mr. E. F. Lange, Mr. M. Longridge, and Mr. E. L. Rhead took part.

Dr. W. E. HOYLE, F.R.S.E., communicated a paper entitled, "**Notes on a Captive Mole**," by LIONEL E. ADAMS, B.A., which was postponed to the next meeting on April 10th.

General Meeting, April 10th, 1906.

Mr. FRANCIS NICHOLSON, F.Z.S., Vice-President, in the Chair.

THE LORD MAYOR OF MANCHESTER (J. H. THEWLIS, Esq.), was elected an ordinary member of the Society.

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accessions to the Society's Library :—"*Bibliotheca Chemica: a Catalogue of the Alchemical, Chemical, and Pharmaceutical Books in the Collection of the late James Young, of Kelly and Durris . . .*", by John Ferguson, 2 vols. (4to., Glasgow, 1906), presented by the Trustees of Dr. J. Young; "*The Physical Geography, Geology, Mineralogy and Paleontology of Essex County, Massachusetts*, by J. H. Sears (4to., Salem., Mass., 1905), presented by the Essex Institute; "*Catalogue of 1772 Stars, chiefly comprised within the Zone 85°—90° N.P.D., for the Epoch 1900 . . . made at the Ratcliffe Observatory, Oxford . . . under the direction of A. A. Rambaut*" (4to., Oxford, 1906), presented by the Radcliffe Trustees; "*New Reduction of Groombridge's Circumpolar Catalogue for the Epoch 1810·0*," by F. W. Dyson and W. G. Thackeray (4to., Edinburgh, 1905), "*Telegraphic Determinations of Longitude made in . . . 1888—1902*," (4to., Edinburgh, 1906), presented by the Royal Observatory, Greenwich.

Ordinary Meeting, May 8th, 1906.

The President, Sir WILLIAM H. BAILEY, in the Chair.

The thanks of the members were voted to the donors of the books upon the table. The following were among the recent donations to the Society's Library :—"*Man's Responsibility*," by T. G. Carson (8vo., New York and London, 1905), presented by the author; "*Collected Mathematical Works of G. W. Hill*," vol. 3, (4to., Washington, 1906), presented by the author through the Carnegie Institution; and "*Zelandia illustrata . . .* " 4°. vervolg, [by] M. Fokker (8vo., Middelburg, 1905).

Mr. FRANCIS NICHOLSON, F.Z.S, presented to the Society's Library a volume of scientific memoirs which had been successively in the possession of Dr. Dalton, F.R.S., Eaton Hodgkin-

Annual General Meeting, April 24th, 1906.

Professor W. BOYD DAWKINS, D.Sc., F.R.S., in the Chair.

The Annual Report of the Council and the Statement of Accounts were presented, and it was resolved:—"That the Annual Report, together with the Statement of Accounts, be adopted, and that they be printed in the Society's *Proceedings*."

Mr. J. BOYD and Mr. CHARLES LEIGH were appointed Scrutineers of the balloting papers.

The following members were elected officers of the Society and members of the Council for the ensuing year:—

President: Sir WILLIAM H. BAILEY.

Vice-Presidents: H. B. DIXON, M.A., F.R.S.; HORACE LAMB, LL.D., D.Sc., F.R.S.; W. BOYD DAWKINS, D.Sc., F.R.S.: FRANCIS NICHOLSON, F.Z.S.

Secretaries: FRANCIS JONES, M.Sc., F.R.S.E., F.C.S.; F. W. GAMBLE, D.Sc.

Treasurer: ARTHUR MCDOUGALL, B.Sc.

Librarian: W. E. HOYLE, M.A., D.Sc., F.R.S.E.

Other Members of the Council: W. THOMSON, F.R.S.E., F.I.C.; THOMAS THORP, F.R.A.S.; CHARLES BAILEY, M.Sc., F.L.S.; R. L. TAYLOR, F.C.S., F.I.C.; CHARLES OLDHAM, ERNEST F. LANGE, F.C.S.

Ordinary Meeting, April 24th, 1906.

Professor W. BOYD DAWKINS, D.Sc., F.R.S., in the Chair.

The thanks of the members were voted to the donors of the books upon the table. The following were among the recent

Annual Report of the Council, April, 1906.

The Society began the session with an ordinary membership of 155. During the present session 14 new members have joined the Society; 17 resignations have been received, and there has been one death, *viz.:* Mr. CHARLES J. HEYWOOD. This leaves on the roll 151 ordinary members. The Society has also lost by death 2 honorary members, *viz.:* Dr. S. P. LANGLEY, Secretary of the Smithsonian Institution, Washington, and Sir J. S. BURDON SANDERSON, F.R.S. Memorial notices of these gentlemen appear at the end of this report.*

The Society commenced the session with a balance in hand of £351. 5s. 6d., from all sources, this amount being made up of the following balances:—

At the credit of General Fund	£18	5	4
„ „ Wilde Endowment Fund...	233	16	6
„ „ Joule Memorial Fund.....	64	16	6
„ „ Dalton Tomb Fund	34	6	2
	£351	5	6

The total balance in hand at the close of the session amounted to £426. 10s. 5d., and the amounts standing at the credit of the separate accounts, on the 31st March, 1906, are the following:—

* Memorial notices of Dr. George Wilson, formerly an ordinary member, Professor Carl Gegenbaur, of Heidelberg, and Mr. Robert Rawson, F.R.A.S., honorary members of this society, appear with the others in this volume. The first two of these were received too late for insertion in the volume of last year, whilst the death of Mr. Rawson was not known at the time of the preparation of this report.

son, F.R.S., and Sir William Fairbairn, Bart., F.R.S., who were Presidents of this Society from 1817—1844, 1848—1850, and 1855—1859 respectively. The dates of the memoirs range from 1822—1830.

The thanks of the meeting were accorded to Mr. NICHOLSON for his interesting gift.

Miss M. C. STOPES, Ph.D., D.Sc., read a paper, entitled "**A New Fern from the Coal Measures** *Tubicaulis Sutcliffii*, spec. nov.

Dr. W. E. HOYLE, F.R.S.E., read a paper written in conjunction with Dr. J. H. ASHWORTH, and entitled "**The Species of Ctenopteryx, a genus of Dibranchiate Cephalopoda.**"

"That before every Annual Meeting the out-going Council "shall prepare ballot papers containing the names of "Members whom they have nominated for election into "the Council and for the various Offices. These ballot "papers to contain the names of five more Members "than there are vacancies to be filled, due regard being "taken that the various Literary and Philosophical "interests of Manchester are adequately represented on "the ballot papers. The President and Vice-Presidents "shall, as far as possible, be selected for nomination "from the out-going Members of Council.

"A notice to be printed on the ballot papers "pointing out that, while other names may be substituted "for those nominated by the Council, the total number "voted for must not exceed the number of vacancies, "otherwise, the ballot paper will be considered void."

Mr. FRANCIS NICHOLSON, F.Z.S., presented to the Society's Library the following works :—

"Annals of Electricity, Magnetism, and Chemistry." Conducted by William Sturgeon. Vols. 1—9. 1837—1842.

"The Annals of Philosophical Discovery and Monthly Reporter of the Progress of Practical Science." Conducted by William Sturgeon. 1 vol. 1843.

Mr. R. F. GWYTHER, M.A., read a paper entitled, "**On the Range of Progressive Waves of Finite Amplitude in Deep Water.**" The paper was communicated by Professor H. LAMB, LL.D., D.Sc., F.R.S.

At this point the Chair was taken by Mr. FRANCIS NICHOLSON, F.Z.S.

Mr. A. D. DARBISHIRE, M.A., B.Sc., read a paper entitled, "**On the Difference between Physiological and Statistical Theories of Heredity.**"

At the credit of General Fund	£69	18	1
„ „ Wilde Endowment Fund ...	248	16	1
„ „ Joule Memorial Fund	72	11	4
„ „ Dalton Tomb Fund	35	4	2
Balance 31st March, 1906.....	£426	10	5

The Wilde Endowment Fund, which is kept as a separate banking account shows a balance of £248 16s. 1d. in its favour, as against £233. 16s. 6d., at the beginning of the financial year, the receipts from the invested funds being the same as last year.

No expenditure has been incurred in respect of the Joule Memorial Fund and the Dalton Tomb Fund, the balances of which remain at the amounts stated above. The Dalton Tomb Fund stands as a separate account at the Manchester and Salford Savings Bank.

The Librarian reports that during the session 757 volumes have been stamped, catalogued and pressmarked, 690 of these being serials, and 67 separate works. There have been written 466 catalogue cards, 365 for serials, and 101 for separate works. The total number of volumes catalogued to date is 29,122 for which 10,195 cards have been written.

Satisfactory use is made of the library for reference purposes, but the number of volumes consulted is not recorded. During the session, 160 volumes have been borrowed from the library, as compared with 134 in the previous session.

Some attention has continued to be paid to the completion of sets, 7 volumes or parts having been obtained, which partly complete two sets. These were presented by the societies publishing them.

A larger amount of binding has been done this session, 498 volumes having been bound in 418.

A record of the accessions to the library shows that, from April, 1905, to March, 1906, 737 serials and 58 separate works were received, a total of 795 volumes. The donations during the session (exclusive of the usual exchanges) amount to 53 volumes and 299 dissertations; 5 volumes have been purchased (in addition to the periodicals on the regular subscription list).

During the past session the Society has arranged to exchange publications with the following:—The Bureau of Standards, Washington; The Field Columbian Museum, Chicago; The Direccion General de Estadistica de la Provincia de Buenos Aires, La Plata; The Washington Academy of Science; The Ethnolcgical Survey for the Phillipine Islands, Manila; The University of Washington, Seattle; The Laboratoire Russe de Zoologie, Villefranche-sur-Mer, and the Academica Polytechnica, Porto.

The publication of the *Memoirs and Proceedings* has been continued under the supervision of the Editorial Committee.

The Society is indebted to the following gentlemen for the undermentioned gifts:—

Mr. Alfred Brothers, F.R.A.S., for a portrait of the late Rev. William Gaskell, and for a lantern slide photographic portrait of Dr. Joule.

Mr. Charles Bailey, M.Sc., F.L.S., for a quarto MS. volume of the minutes of the meetings of the Manchester Botanists' Association.

Mr. Francis Nicholson, F.L.S., for "Annals of Electricity, Magnetism, and Chemistry," conducted by W. Sturgeon. Vols. 1—9, 1837-42, and "The Annals of Philosophical Discovery," conducted by W. Sturgeon. 1 vol., 1843.

The Council arranged for the Wilde Lecture to be delivered on Tuesday, March 20th, 1906, by Professor H. H. Turner, D.Sc., F.R.S., of Oxford University.

The Council resolved that an address be presented to the

American Philosophical Society on the occasion of the celebration of the 200th anniversary (April 17-20, 1906) of the birth of Benjamin Franklin, and that Dr. F. W. Clarke, Honorary Member of this Society, be requested to act as its representative. The following is a copy of the address:—

To The American Philosophical Society.

The Manchester Literary and Philosophical Society sends greetings to its sister, The American Philosophical Society for Promoting Useful Knowledge, on the occasion of the Two Hundredth Anniversary of the birth of its founder, Benjamin Franklin.

As philosopher, statesman and diplomatist, and as a pioneer in the scientific fields of capillarity, acoustics, electricity and meteorology, Dr. Franklin will long be remembered, and his intimate association with your Society is a circumstance of which you may be justly proud.

(Signed) W. H. BAILEY, K.B., *President.*

FRANCIS JONES, } *Hon.*
CHARLES H. LEES, } *Secretaries.*

April 6th, 1906.

Sir J. S. BURDON-SANDERSON, Bart., F.R.S. was born at Jesmond near Newcastle-on-Tyne in December, 1828, and was educated at home. His family on both sides bore names of distinction which have formed a study in eugenics by Francis Galton. The border country made a strong appeal to Sanderson's nature, and throughout life he revelled in moorland scenery and wild life.

Sanderson entered the University of Edinburgh as a medical student, and soon displayed an inborn faculty for physiological research. Whilst still an undergraduate he finished two papers on vegetable irritability and on the metamorphosis of coloured

General Meeting, March 27th, 1906.

The President, Sir WILLIAM H. BAILEY, in the Chair.

The following resolution, proposed by Mr. C. E. STROMEYER, M.Inst.C.E., and seconded by Mr. E. F. LANGE, was carried :—

"That before every Annual Meeting the out-going Council "shall prepare ballot papers containing the names of "Members whom they have nominated for election into "the Council and for the various Offices. These ballot "papers to contain the names of two new Members, "due regard being taken that the various Literary and "Philosophical interests of Manchester are adequately "represented on the ballot papers. The President and "Vice-Presidents shall, as far as possible, be selected "for nomination from the out-going Members of "Council.

"A notice to be printed on the ballot papers "pointing out that, while other names may be substituted "for those nominated by the Council, the total number "voted for must not exceed the number of vacancies, "otherwise, the ballot paper will be considered void."

Ordinary Meeting, March 27th, 1906.

The President, SIR WILLIAM H. BAILEY, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Mr. C E. STROMEYER, M.Inst.C.E., read a paper entitled, **"On Recent Mysterious Fractures of Steel Plates."**

The author briefly referred to the well-known failures of steel plates of the boilers of the Imperial Russian Yacht "Livadia," and to Mr. Maginnis's experiences with two brittle marine boilers,

in fish are classic. The Oxford Medical School is his enduring monument, and in the minds of all who knew him Burdon-Sanderson's personality, striking, attractive, inspiring, will colour the memory of his achievements.

Sir John Burdon-Sanderson received many honours from learned Societies during his lifetime. He was thrice Croonian Lecturer, twice for the Royal Society, in 1867 and 1877, and once for the Royal College of Physicians, in 1891. In 1878 he was Harveian Orator for the College of Physicians, and in 1880 received the Baly Medal. For special research work and for his general services to physiology and pathology he was awarded in 1883 one of the Royal Medals of the Royal Society, of which institution he was a Fellow.

He served on three Royal Commissions—that on Hospitals in 1883, that on the Consumption of Tuberculous Meat and Milk in 1890, and that on the University of London, 1892-94. In 1893, Sir J. Burdon-Sanderson was President of the Nottingham meeting of the British Association. He was elected an Honorary Member of the Manchester Literary and Philosophical Society in 1894. F. W. G.

Professor GEGENBAUR was born in Würzburg on August 21st, 1826. He attended the local "Gymnasium," and in 1845 matriculated in the University of his native town as a student of medicine and natural science. Under the guidance of such distinguished leaders as Kölliker, Virchow, Leydig, and Heinrich Müller, Gegenbaur acquired a thorough grasp of the comparative method, and his first post-graduate work was an investigation into the life-histories of Medusæ and other marine organisms, which he carried out partly in the North Sea partly in the Mediterranean in the stimulating company of Johannes Müller and Kölliker. For two years Gegenbaur continued to work out the marine zoology of Italy, and in this period much of his grasp of invertebrate anatomy was acquired.

In 1855 Gegenbaur was made Professor of Zoology and allied subjects in the University of Jena, but in 1862, upon the re-

General Meeting, March 27th, 1906.

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for much of the present knowledge of vertebrate and invertebrate zoology. But greater than his writings or his influence was the dominant character of the man himself. A short autobiography appeared in 1901. F. W. G.

SAMUEL PIERPONT LANGLEY, who has been one of our honorary members since 1887, died on February 27th, 1906, at the age of 72, while still in possession of all his faculties. A brief mention of the more important of his contributions to scientific literature is all that can be attempted within the limits at disposal. By the invention of the bolometer he was enabled to study the heat radiation from point to point of the sun's disc, to estimate anew the constant of solar radiation, and to map out the solar spectrum on the infra-red side, as far as ·76μ. More recently the investigation has been continued at the Allegheny Observatory, under his direction, and with an improved form of the instrument, to a still lower limit, ·37μ. The discovery that there are considerable gaps in this portion of the spectrum, due to atmospheric absorption, is likely to prove of service to meteorologists. As an observer and delineator of solar phenomena, Langley has probably never been surpassed, while his determination of the moon's temperature is a model of patient work carried out under most baffling conditions. To the problem of aerial flight he also gave much attention, and his aerodrome doubtless embodies the principles upon which, if ever, the true conquest of the air will be achieved. In addition to the highest experimental skill and ingenuity, Langley possessed in an unusual degree the qualities of an organiser and administrator, the foundation of the Smithsonian Astrophysical Observatory, and of the National Zoological Park being entirely due to his efforts, and his eighteen years secretaryship of the Smithsonian Institution testifies still further to his many-sided vigour. C. L. B.

ROBERT RAWSON was born at Brinsley, Nottinghamshire, July 22nd, 1814. Up to the age of 23 he worked as a miner, but having acquired a good knowledge of mathematics he then

undertook the duties of clerk and draughtsman in a railway constructor's office, where he was engaged from 1837 to 1842. Then for some years he was resident in Manchester as a private teacher of mathematics. He was elected a member of the Society in 1845, and was on the Council in 1847, in which year he left Manchester, having accepted the offer of the Admiralty of the position of Head Master of the Dockyard Schools, Portsmouth. He was elected an Honorary Member of the Society in 1849.

He retired from the Dockyard Schools in 1875, when he was the recipient of a Testimonial which was presented to him by Admiral Maclintock, K.C.B., F.R.S., LL.D. He was a Fellow of the Royal Astronomical Society, and an Associate Member of the Society of Naval Architects from its commencement.

His communications to this Society, chiefly on mathematical subjects, numbered fifteen, and extended over the long period of nearly forty years. The first appeared in 1844, and the last in 1883.

He died at his residence, Warblington Villa, Havant, March 11th, 1906. T. T.

CHARLES JAMES HEYWOOD, who was elected an ordinary member on January 8th, 1889, was one of a family connected with this Society for more than a hundred years. A remote ancestor of our late member was the Rev. Nathaniel Heywood, M.A., ejected in 1662, for nonconformity, from the Vicarage of Ormskirk. From the son of the ejected Vicar, also the Rev. Nathaniel Heywood, M.A., Nonconformist Minister at Ormskirk, descended two or three generations of Irish and Liverpool merchants. The family established a bank in Liverpool, and later on in 1788 two members of the family started the banking business in Manchester.

In the following year began the long connection of the family with the Literary and Philosophical Society by the election to membership on February 6th, 1789, of Mr. Benjamin Heywood, as he is called in the list of members, though it should doubtless

have been Benjamin Arthur Heywood. B. A. Heywood was elected Treasurer in 1791, and was succeeded in that office by his brother Nathaniel. Though he was Treasurer from 1796 to 1814, Nathaniel Heywood's name does not occur in the list of members. He married Ann, daughter of Thomas Percival, M.D., F.R.S., the virtual founder of the Society, its first Vice-President, and for many years its President. Three of Nathaniel's sons were members of the Society, Benjamin (elected 1815), Richard (elected 1822), and James (elected 1833). The last was a F.R.S., and M.P. for North Lancashire, and is well-known for his successful efforts to open the older universities to non-conformists. The eldest son Benjamin, like his brother James, was a F.R.S. and a Member of Parliament, and in 1838 was created a baronet. He served the Society as Treasurer from 1815 to 1850. He continued the traditions of his family in his acceptance of the responsibilities of wealth, and in the first half of the last century took part in every movement for the amelioration of the condition of his fellow citizens. Sir Benjamin's second son, Oliver Heywood, joined the Society in 1864. By his public spirit and charity he well earned the honorary freedom of the city, which was conferred on him in 1888, he being the first person so honoured, and he is one of the very few Manchester men commemorated by a statue in his native city. Charles James Heywood, Oliver Heywood's younger brother, and sixth son of Sir Benjamin, was born at Acresfield, Pendleton, March 25th, 1835, and died at Chasely, Pendleton, December 1st, 1905. He was educated at Harrow, and at Trinity College, Cambridge, and graduated B.A. in 1856, and M.A. in 1859. Soon after leaving the University he became a partner in the family banking business and was associated with it until Heywood's Bank was absorbed by the Manchester and Salford Bank. In 1858 Mr. Heywood married Anna Margaret, daughter of William Langton, F.S.A., a well-known antiquary and banker, and a member of the Society. Mrs. Heywood survives her husband.

In strictly municipal affairs Mr. Heywood took no part, but he was a generous supporter of local charities, and had an active

share in their management. He was a Churchman of the liberal Anglican type and for very many years was a Sunday School teacher. Of the Gentlemen's Concerts he was Treasurer and a Director, and at one crisis in the history of the Concerts came to their rescue by buying the Gentlemen's Concert Hall and letting it to the Directors at a nominal rent. When the Midland Railway Company bought the site of the Hall for their Midland Hotel, instead of making for himself a handsome profit on the transaction, he was content to stipulate that there should be accommodation for the Gentlemen's Concerts provided in the Hotel.

Mr. Heywood was a man of charming personality, and was a worthy member of a family remarkable during many generations for its high character, public spirit, and great beneficence. Though he never took any personal part in the work of the Society, still we must all feel regret that by Mr. Heywood's death a family name that has ever been held in high esteem now disappears from our list of members.

F. N.

By the death of Dr. GEORGE WILSON, which took place on February 16th, 1905, engineering science in this country has lost one of its most promising workers, and the Society one of its most active contributors. Dr. Wilson was born on February 17th, 1871, being the only son of Mr. George Wilson, of Manchester, and grandson of Mr. George Wilson who was chairman of the Anti Corn Law League, and chairman of directors of the Lancashire and Yorkshire Railway Company. He was educated privately, and at The Owens College, which he entered in 1887, passing through the engineering course, and graduating with First Class Honours in Engineering in 1891. After spending two years in the offices of the Manchester Ship Canal, he was appointed Junior Demonstrator in Engineering in The Owens College, relinquishing this appointment to become Lecturer in University College, Cardiff, in 1895. In 1896 he was offered the post of Senior Demonstrator at Manchester, together with a

Resident Tutorship at Hulme Hall, which posts he held until 1904, when he was appointed Lecturer on Civil Engineering in the University of Leeds. He obtained the D.Sc. degree in the Victoria University in 1900.

Dr. Wilson was a most effective and popular teacher, and his powers of organisation were displayed to a marked degree in the successful conduct of the large classes in the Whitworth Laboratory. As an investigator he achieved great success in that field of engineering research which requires the application of mathematical analysis of an advanced character, and it was this application which was undoubtedly the strongest feature of his work. Although he had barely attained the age of 34, he had published the results of a considerable number of investigations which he had successfully carried out, and there can be no doubt that his early death cut short a career of the highest promise. His loss will be widely regretted by the large number of engineers who have had the good fortune to attend his classes in the Whitworth Laboratory, and still more by those of his contemporaries who have been indebted to him for his invaluable advice and help in professional matters, aid which was always readily and ungrudgingly bestowed.

Of the considerable number of papers on engineering subjects which he has published, the following may be mentioned:

"On the determination of the reaction at the points of support of Continuous Beams." *Proc. Royal Society*, vol. 62.

"On the relation between Uniform Stress and Permanent Strain in Annealed Copper Bars and Wires." *Manchester Memoirs*, vol. 43, part 4.

"Experiments on Conjugate Pressures in Fine Sand." *Proc. Inst. Civil Engineers*, vol. 149, part 3.

"On the Failure of certain Cast Steel Dies used in the manufacture of Drawn Tubes." *Manchester Memoirs*, vol. 46, part 2.

"A Factor in the safety of High Speed Torpedo Boat Destroyers." *Manchester Memoirs*, vol. 47, part 5.

T. E. S.

NOTE.—The Treasurer's Accounts of the Session 1905-1906, of which the following pages are summaries, have been endorsed as follows:

April 12th, 1906. Audited and found correct.

We have also seen, at this date, the certificates of the following Stocks held in the name of the Society:—£1,225 Great Western Railway Company 5% Consolidated Preference Stock, Nos. 12,293, 12,294, and 12,323; £258 Twenty years' loan to the Manchester Corporation, redeemable 25th March, 1914 (No. 1564); £7,500 Gas Light and Coke Company Ordinary Stock (No. 6,389); and the deeds of the Natural History Fund, of the Wilde Endowment Fund, those conveying the land on which the Society's premises stand, and the Declaration of Trust.

Leases and Conveyance dated as follow:—

22nd Sept., 1797.
23rd Sept., 1797.
25th Dec., 1799.
" " "
22nd Dec., 1820.
23rd Dec., 1820.

Declarations of Trust:—

8th Jan., 1878.
24th June, 1801.
23rd Dec., 1820.
30th April, 1851.

We have also verified the balances of the various accounts with the bankers' pass books.

(Signed) R. S. HUTTON.
C. S. ALLOTT.

MANCHESTER LITERARY AND

Dr. *Arthur McDougall, Treasurer, in Account with the*

			£ s. d.	£ s. d.
To Cash in hand, 1st April, 1905				83 18
To Members' Subscriptions :—				
Half Subscriptions, 1901-02,	1 at £1 1s. od.		1 1 0	
,, ,, 1903-04,	1 ,, ,,		1 1 0	
,, ,, 1904-05,	3 ,, ,,		3 3 0	
,, ,, 1905-06,	13 ,, ,,		13 13 0	
Subscriptions :— 1900-01,	1 ,, £2. 2s. od.		2 2 0	
,, 1902-03,	1 ,, ,,		2 2 0	
,, 1903-04,	2 ,, ,,		4 4 0	
,, 1904-05,	15 ,, ,,		31 10 0	
,, 1905-06,	118 ,, ,,		247 16 0	
,, 1906-07,	1 ,, ,,		2 2 0	
				328 14 0
To Transfers from the Wilde Endowment Fund			.. ..	70 1 0
To Sale of Publications			.. ..	13 3 7
To H. Sidebottom for Plates			.. ..	2 2 0
To Dividends :—				
Natural History Fund			58 3 9	
Joule Memorial Fund			7 7 2	
				65 10 11
To Income Tax Refunded :—				
Natural History Fund			3 1 4	
Joule Memorial Fund			0 7 8	
				3 9 0
To Donation from Mr. H. Bolton ..			.. ..	2 2 1
To Discount on Knowles' bill ..			.. ..	0 0 2
				£577 16 1

NATURAL HISTORY

	£ s. d.
To Balance 1st April, 1905	6 16 1
To Dividends on £1,225 Great Western Railway Company's Stock	58 3 1
To Remission of Income Tax, 1905	3 1 6
	£68 1 1

JOULE MEMORIAL

	£ s. d.
To Balance, 1st April, 1905	64 16 1
To Dividends on £258 Loan to Manchester Corporation	7 7 1
To Remission of Income Tax, 1905	0 7 8
	£72 11 6

WILDE ENDOWMENT

	£ s. d.
To Balance 1st April, 1905	233 16 6
To Dividends on £7,500 Gas Light and Coke Company's Ordinary Stock	313 10 6
To Remission of Income Tax, 1905	18 10 6
To Bank Interest	2 14 6
To Legal Expenses refunded	1 1 6
	£567 13 6

DALTON TOM

	£ s. d.
To Balance, 1st April, 1905	34 7 1
To Bank Interest	0 17 6
	£35 4 7

PHILOSOPHICAL SOCIETY.

Society, from 1st April, 1905, to 31st March, 1906. Cr,

	£ s. d.	£. s. d.
By Charges on Property :—		
Chief Rent (Income Tax deducted)	12 5 4	
Income Tax on Chief Rent	0 12 11	
Insurance against Fire	13 17 6	
By House Expenditure :—		26 15 9
Coals, Gas, Electric Light, Water, Wood, &c.	32 14 1	
Tea, Coffee, &c., at Meetings	15 13 0½	
Cleaning, Sweeping Chimneys, &c.	5 13 11	
Crockery	1 15 3	
By Administrative Charges :—		55 16 3½
Housekeeper	62 8 0	
Postages, and Carriage of Parcels and of "Memoirs"	32 4 10½	
Stationery, Cheques, Receipts, and Engrossing	9 0 7	
Printing Circulars, Reports, &c.	10 8 9	
Extra attendance at Meetings, and during housekeeper's holidays	3 6 6	
Miscellaneous Expenses	3 18 0	
		121 6 8½
By Publishing :—		
Printing "Memoirs and Proceedings"	88 0 10	
Illustrations for "Memoirs" (except Nat. Hist. papers)	0 6 0	
Binding "Memoirs"	3 6 0	
By Library :—		91 12 10
Books and Periodicals (except those charged to Natural History Fund) ..	48 15 9	
Periodicals formerly subscribed for by the Microscopical and Natural History Section	4 14 0	
By Natural History Fund :—		53 9 9
(Items shown in the Balance Sheet of this Fund below)	.. ..	66 4 9
By Joule Memorial Fund :—		
(No Expenditure this Session)		0 0 0
By Balance at Williams Deacon's Bank, 1st April, 1906	132 10 2	
,, ,, in Treasurer's hands	10 0 0	
		142 10 2
		£557 16 3

FUND, 1905—1906. (Included in the General Account, above.)

	£ s. d.
By Natural History Books and Periodicals	51 6 5
By illustrations for papers on Nat. Hist. in "Memoirs"	14 18 4
,, Balance, 1st April, 1906	1 16 9
	£68 1 6

FUND, 1905—1906. (Included in the General Account, above.)

	£ s. d.
(No expenditure this Session).	
By Balance, 1st April, 1906	72 11 4
	£72 11 4

FUND, 1905—1906.

	£ s. d.
By Assistant Secretary's Salary, April, 1905, to March, 1906	135 0 0
By Maintenance of Society's Library :—	
Binding and Repairing Books	61 11 10
By Repairs to Society's Premises	8 4 9
By Cleaning Carpets..	3 2 4
By Legal Expenses	15 11 6
By Honorarium to Lecturer. 1906	15 15 0
By Transfers to Society's Funds	79 8 0
By Cheque Book	0 2 6
By Balance at District Bank, 1st April, 1906	248 16 1
	£567 12 0

FUND, 1905-1906.

	£ s. d.
(No Expenditure this Session).	
By Balance at Manchester and Salford Savings Bank, 1st April, 1906	35 4 2
	£35 4 2

THE COUNCIL AND MEMBERS

OF THE

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

(CORRECTED TO AUGUST 14TH, 1906.)

President.

SIR WILLIAM H. BAILEY, M.I.Mech.E.

Vice-Presidents.

H. B. DIXON, M.A., F.R.S., F.C.S.

HORACE LAMB, M.A., LL.D., D.Sc., F.R.S.

W. BOYD DAWKINS, M.A., D.Sc., F.R.S.

FRANCIS NICHOLSON, F.Z.S.

Secretaries.

FRANCIS JONES, M.Sc., F.R.S.E., F.C.S.

F. W. GAMBLE, D.Sc.

Treasurer.

ARTHUR McDOUGALL, B.Sc.

Librarian.

W. E. HOYLE, M.A., D.Sc., F.R.S.E.

Other Members of the Council.

WILLIAM THOMSON, F.R.S.E., F.I.C.

THOMAS THORP, F.R.A.S.

CHARLES BAILEY, M.Sc., F.L.S.

R. L. TAYLOR, F.C.S., F.I.C.

CHARLES OLDHAM.

ERNEST F. LANGE, F.C.S.

Assistant Secretary and Librarian.

A. P. HUNT, B.A.

ORDINARY MEMBERS.

Date of Election.

1901, Dec. 10. Adamson, Harold. *Oaklands Cottage, Godley, near Manchester.*

1902, Mar. 18. Allen, J. Fenwick. 147, *Withington Road, Whalley Range, Manchester.*

1902, Jan. 21. Allott, Charles S., M.Inst.C.E. 519, *Stretford Road, Old Trafford, Manchester.*

1870, Dec. 13. Angell, John, F.C.S., F.I.C. 6, *Beaconsfield, Derby Road, Withington, Manchester.*

1896, Jan. 31. Armstrong, Frank. 88, *Deansgate, Manchester.*

1865, Nov. 14. Bailey, Charles, M.Sc., F.L.S. *Atherstone House, North Drive, St. Annes-on-the-Sea, Lancs.*

1888, Feb. 7. Bailey, Alderman Sir William H., M.I.Mech.E. *Sale Hall, Sale, Cheshire.*

1895, Jan. 8. Barnes, Charles L., M.A. 8, *Swinton Avenue, Chorlton-on-Medlock, Manchester.*

1903, Oct. 20. Barnes, Jonathan, F.G.S. *South Cliff House*, 301, *Great Clowes Street, Higher Broughton, Manchester.*

1896, April 14. Behrens, George B. *Caerfedwen, Trefnant, North Wales.*

1895, Mar. 5. Behrens, Gustav. *Holly Royde, Withington, Manchester.*

1898, Nov. 29. Behrens, Walter L. 22, *Oxford Street, Manchester.*

1868, Dec. 15. Bickham, Spencer H., F.L.S. *Underdown, Ledbury.*

1901, Nov. 12. Bles, Abraham J. S. *Palm House, Higher Broughton, Manchester.*

1896, Oct. 6. Bowman, F.H., D.Sc., F.R.S.E. 4, *Albert Square, Manchester.*

1896, Feb. 18. Bowman, George, M.D. 594, *Stretford Road, Old Trafford, Manchester.*

1875, Nov. 16. Boyd, John. *Barton House*, 11, *Didsbury Park, Didsbury, Manchester.*

1902, Oct. 21. Bradley, Henry Wentworth. *Woodside, Wilmslow, Cheshire.*

1889, Oct. 15. Bradley, Nathaniel, F.C.S. *Sunnyside, Whalley Range, Manchester.*

Date of Election.

1861, April 2. Brogden, Henry, F.G.S., M.I.Mech.E. *Hale Lodge, Altrincham, Cheshire.*

1889, April 16. Brooks, Samuel Herbert. *Slade House, Levenshulme, Manchester.*

1860, Jan. 24. Brothers, Alfred. *Handforth, near Manchester.*

1886, April 6. Brown, Alfred, M.A., M.D. *Sandycroft, Higher Broughton, Manchester.*

1889, Jan. 8. Brownell, Thomas William, F.R.A.S. 64, *Upper Brook Street, Manchester.*

1889, Oct. 15. Budenberg, C. F., M.Sc., M.I.Mech.E. *Bowdon Lane, Marple, Cheshire.*

1905, Nov. 14. Burgess, Charles H., M.Sc., Demonstrator of Chemistry in the University of Manchester. *Shaftesbury House, Cheadle Hulme, Manchester.*

1906, Feb. 27. Burton, Joseph, A.R.C.S., Dublin. *Tile Works, Clifton Junction, near Manchester.*

1894, Nov. 13. Burton, William, F.C.S. *The Hollies, Clifton Junction, near Manchester.*

1904, Oct. 18. Campion, George Goring, L.D.S. 264, *Oxford Street, Manchester.*

1903, Nov. 3. Capper, Stewart Henbest, M.A., Professor of Architecture in the Victoria University of Manchester. 337, *Moss Lane East, Manchester.*

1899, Feb. 7. Chapman, D. L., M.A., Assistant Lecturer and Demonstrator of Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1901, Nov. 26. Chevalier, Reginald C., M.A., Mathematical Master at the Manchester Grammar School. 43, *Lansdowne Road, West Didsbury, Manchester.*

1902, Nov. 4. Clerk, Dugald, M.Inst.C.E., F.C.S. 18, *Southampton Buildings, Chancery Lane, London, W.C.*

1901, Nov. 12. Coignou, Caroline, M.A., Science Mistress at the Manchester High School for Girls. 60, *Cecil Street, Greenheys, Manchester.*

1895, April 30. Collett, Edward Pyemont. 8, *St. John Street, Manchester.*

1884, Nov. 4. Corbett, Joseph. *Town Hall, Salford.*

1903, Oct. 20. Core, William Hamilton, M.Sc. *Groombridge House, Withington, Manchester.*

1895, Nov. 12. Crossley, W. J., M.I.Mech.E. *Openshaw, Manchester.*

Date of Election.

1904, Oct. 18. Crosthwaite, Robert, M.A. Camb., B.Sc. Lond., Head Master of the Municipal Secondary School, Whitworth Street. 25, *Rathen Road, Withington, Manchester.*

1904, Jan. 5. Darbishire, Arthur D., M.A., B.Sc. 31, *Eardley Crescent, London, S.W.*

1901, Nov. 26. Darbishire, Francis V., B.A., Ph.D., Demonstrator and Analyst at the South Eastern Agricultural College. *The College, Wye, Kent.*

1895, April 9. Dawkins, W. Boyd, M.A., D.Sc., F.R.S., Professor of Geology in the Victoria University of Manchester. *Fallowfield House, Fallowfield, Manchester.*

1894. Mar. 6. Delépine, A. Sheridan, M.B., B.Sc., Professor of Pathology in the Victoria University of Manchester. *The University, Manchester.*

1887, Feb. 8. Dixon, Harold Baily, M.A., M.Sc., F.R.S., F.C.S., Professor of Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1905, Jan. 10. Duffield, W. Geoffrey, B.A., B.Sc., Research Fellow in the University of Manchester. *The University, Manchester.*

1906, Jan. 30. Dunkerley, Stanley, D.Sc., Professor of Engineering in the University of Manchester. *The University, Manchester.*

1902, May 13. Ellison, Robert William. *'Brookside,' Crofts Bank Road, Urmston, Manchester.*

1883, Oct. 2. Faraday, F. J., F.L.S., F.S.S. *Carshalton House, Heaton Road, Withington, Manchester.*

1905, May 2. Fearon, Ernest, Chemist to the Salford Corporation Gas Works. *Hundon House, Arnold Road, Alexandra Park, Manchester.*

1903, Dec. 15. Fishenden, Richard B., Lecturer in Photo-Mechanical Processes at the Municipal School of Technology, Manchester. 311, *Moss Lane East, Manchester.*

1898, Nov. 29. Gamble, F. W., D.Sc., Assistant Lecturer and Demonstrator of Zoology in the Victoria University of Manchester. *The University, Manchester.*

1896, Nov. 17. Gordon, Rev. Alexander, M.A. *Summerville, Victoria Park, Manchester.*

1905, Nov. 20. Grimshaw, William Edwin, B.A., Physics Master, Manchester Grammar School. 46, *Broadway Street, Oldham.*

Date of Election.

1902, April 29. Herbert, Arthur M., B.A. *Frankwyn, Hale, Cheshire.*

1905, Oct. 17. Hewitt, Charles Gordon, B.Sc., Assistant Lecturer and Demonstrator in Zoology in the Victoria University of Manchester. *The University, Manchester.*

1902, Jan. 7. Hewitt, David B., M.D. *Oakleigh, Northwich, Cheshire.*

1895, Mar. 5. Hickson, Sydney J., M.A., D.Sc., F.R.S., Professor of Zoology in the Victoria University of Manchester. *The University, Manchester.*

1884, Jan. 8. Hodgkinson, Alexander, M.B., B.Sc. 18, *St. John Street, Manchester.*

1905. Nov. 14. Holt, Alfred, M.A., Research Fellow of the University of Manchester. *Crofton, Aigburth, Liverpool.*

1898, Nov. 29. Hopkinson, Alfred, K.C., M.A., LL.D., Vice-Chancellor of the Victoria University of Manchester. *Fairfield, Victoria Park, Manchester.*

1896, Nov. 3. Hopkinson, Edward, M.A., D.Sc., M.Inst.C.E. *Ferns, Alderley Edge, Cheshire.*

1889, Oct. 15. Hoyle, William Evans, M.A., D.Sc., F.R.S.E., Director of the Manchester Museum. *The University, Manchester.*

1900, Oct. 16. Hutton, R. S., D.Sc., Lecturer on Electro-Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1899, Oct. 17. Ingleby, Joseph, M.I.Mech.E. *Summer Hill, Pendleton, Manchester.*

1901, Nov. 26. Jackson, Frederick. 14, *Cross Street, Manchester.*

1870, Nov. 1. Johnson, William H., B.Sc. *Woodleigh, Altrincham.*

1878, Nov. 26. Jones, Francis, M.Sc., F.R.S.E., F.C.S. *Manchester Grammar School*, and *Beaufort House, Alexandra Park, Manchester.*

1886, Jan. 12. Kay, Thomas. *Moorfield, Stockport, Cheshire.*

1895, Nov. 12. Kirkman, William Wright. *The Grange, Timperley, Cheshire.*

1903, Feb. 3. Knecht, Edmund, Ph.D., Professor of Tinctorial Chemistry at the Municipal School of Technology, Manchester. 5, *Station Road, Crumpsall, Manchester.*

1904, Oct. 18. Knowles, H. B., M.A., Principal of the Royal Technical Institute, Salford. *'Norcote,' Swinton Park, Salford.*

1902, Feb. 4. Kolp, Noah. *Woodthorpe, Victoria Park, Manchester.*

Date of Election.

1893, Nov. 14. Lamb, Horace, M.A., LL.D., D.Sc., F.R.S., Professor of Mathematics in the Victoria University of Manchester. 6, *Wilbraham Road, Fallowfield, Manchester.*

1902, Jan. 7. Lange, Ernest F., F.C.S. *Fairholme*, 3, *Willow Bank, Fallowfield, Manchester.*

1904, Mar. 15. Lea, Arnold W. W., M.D. 246, *Oxford Road, Manchester.*

1903, Nov. 17. Leigh, Charles W. E., Librarian of the University. *The University, Manchester.*

1902, Nov. 4. Leigh, Sir Joseph Egerton. *The Towers, Didsbury, Manchester.*

1902, Jan. 7. Longridge, Michael, M.A., M.Inst.C.E. *Linkvretten, Ashley Road, Bowdon, Cheshire.*

1857, Jan. 27. Longridge, Robert Bewick, M.I.Mech.E. *Yew Tree House, Tabley, Knutsford, Cheshire.*

1866, Nov. 13. McDougall, Arthur, B.Sc. *Lyndhurst, The Park, Buxton.*

1905, Oct. 31. McNicol, Mary, B.Sc., Research Scholar in the Victoria University of Manchester. 182, *Upper Chorlton Road, Manchester.*

1904, Nov. 1. Makower, Walter, B.A., B.Sc. 214, *Upper Brook Street, Manchester.*

1902, Mar. 4. Mandleberg, Goodman Charles. *Carlton House, Broom Lane, Higher Broughton, Manchester.*

1875, Jan. 26. Mann, J. Dixon, M.D., F.R.C.P. (Lond.), Professor of Medical Jurisprudence in the Victoria University of Manchester. 16, *St. John Street, Manchester.*

1901, Dec. 10. Massey, Herbert. *Ivy Lea, Burnage, Didsbury, Manchester.*

1864, Nov. 1. Mather, Sir William, M.P., M.Inst.C.E., M.I.Mech.E. *Iron Works, Salford.*

1873, Mar. 18. Melvill, James Cosmo, M.A., F.L.S. *Meole-Brace Hall, Shrewsbury.*

1881, Oct. 18. Mond, Ludwig, D.Sc., Ph.D., F.R.S., F.C.S. *Winnington Hall, Northwich, Cheshire.*

1894, Feb. 6. Mond, Robert Ludwig, M.A., F.R.S.E., F.C.S. *Winnington Hall, Northwich, Cheshire.*

1903, Oct. 20. Moore, Frederick Craven, M.D., M.Sc., 61, *Ardwick Green, Manchester.*

1899, Mar. 7. Morris, Edgar F., M.A., F.C.S. *Grey House, Barrington Road, Altrincham, Cheshire.*

1902, Feb. 18. Moss, William E., B.A. *C/o Messrs. Davies, Benachi & Co., 7, Rumford Street, Liverpool.*

Date of Election.

1873, Mar. 4. Nicholson, Francis, F.Z.S. *The Knoll, Windermere, Westmoreland.*

1900, April 3. Nicolson, John T., D.Sc., Professor of Engineering at the Municipal School of Technology, Manchester. *Nant-y-Glyn, Marple, Cheshire.*

1889 April 16. Norbury, George. *Hillside, Prestwich Park, Prestwich, Lancs.*

1884, April 15. Okell, Samuel, F.R.A.S. *Overley, Langham Road, Bowdon, Cheshire.*

1903, Jan. 6. Oldham, Charles. *Brook Cottage, Knutsford, Cheshire.*

1901, Nov. 26. Paine, Standen. *Devisdale, Bowdon, Cheshire.*

1892, Nov. 15. Perkin, W. H., jun., Ph.D., M.Sc., F.R.S., Professor of Organic Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1901, Oct. 29. Petavel, J. E., B.A. *The University, Manchester.*

1885, Nov. 17. Phillips, Henry Harcourt, F.C.S. *Lynwood, Turton, nr. Bolton, Lancs.*

1902, Oct. 21. Pope, W. J., F.R.S., F.C.S., Professor of Chemistry at the Municipal School of Technology, Manchester. *Corchester, Bramhall, Cheshire.*

1901, Nov. 12. Pratt, Edith M., D.Sc. *Peak House, Dukinfield, Cheshire.*

1903, Dec. 15. Prentice, Bertram, Ph.D., D.Sc., Lecturer in Chemistry, Royal Technical Institute, Salford. *Primrose Villa, Snowdon Road, Eccles.*

1903, Feb. 3. Radcliffe, L. G., F.C.S., Lecturer in Chemistry at the Municipal School of Technology, Manchester. 6, *Alma Terrace, Old Trafford, Manchester.*

1904, Feb. 2. Radford, Catherine, B.Sc. 31, *Cawdor Road, Fallowfield, Manchester.*

1900, Feb. 20. Ragdale, John R. *The Beeches, Whitefield, near Manchester.*

1901, Dec. 10. Ramsden, Herbert, M.D. (Lond.), M.B., Ch.B. (Vict.). *Sunnyside, Dobcross, near Oldham, Lancs.*

1888, Feb. 21. Rée, Alfred, Ph.D., F.C.S. 15, *Mauldeth Road, Withington, Manchester.*

1869, Nov. 16. Reynolds, Osborne, M.A., LL.D., F.R.S., M.Inst.C.E. 19, *Ladybarn Road, Fallowfield, Manchester.*

Date of Election.

1880, Mar. 23. Roberts, D. Lloyd, M.D., F.R.S.E., F.R.C.P. (Lond.). *Ravenswood, Broughton Park, Manchester.*

1897, Oct. 19. Rothwell, William Thomas. *Heath Brewery, Newton Heath, near Manchester.*

1905, Oct. 31. Saxelby, Edith Mary, B.Sc., Research Scholar in the Victoria University of Manchester. 3, *Alexandra Road South, Alexandra Park, Manchester.*

1873, Nov. 18. Schuster, Arthur, Sc.D., Ph.D., F.R.S., F.R.A.S., Professor of Physics in the Victoria University of Manchester. *Kent House, Victoria Park, Manchester.*

1898, Jan. 25. Schwabe, Louis. *Hart Hill, Eccles Old Road, Pendleton, Manchester.*

1902, Jan. 21. Shann, Sir Thomas Thornhill. *Meadow Bank, Heaton Norris, Stockport.*

1890, Nov. 4. Sidebotham, Edward John, M.A., M.B., M.R.C.S. *Erlesdene, Bowdon, Cheshire.*

1903, April 28. Sidebottom, Henry. *The Hall Cottage, Cheadle Hulme, near Stockport.*

1901, Oct. 29. Sinclair, Sir W. J., M.D., Professor of Obstetrics and Gynæcology in the Victoria University of Manchester. *Garvock House, Dudley Road, Whalley Range, Manchester.*

1895, Nov. 12. Southern, Frank, B.Sc. 6, *Park Avenue, Timperley, Cheshire.*

1896, Feb. 18. Spence, David. *Honeyhanger, Haslemere, Surrey.*

1901, Dec. 10. Spence, Howard. *Audley, Broad Road, Sale, Cheshire.*

1904, Nov. 1. Stansfield, Herbert, B.Sc., A.I.E.E. 20, *Every Street, Ancoats, Manchester.*

1905, May 2. Stopes, Marie C., D.Sc., Ph.D., Demonstrator of Botany in the University of Manchester. 11, *Kensington Avenue, Victoria Park, Manchester.*

1897, Nov. 30. Stromeyer, C. E., M.Inst.C.E. *Steam Users' Association, 9, Mount Street, Albert Square, Manchester.*

1905, Nov. 1. Sutcliffe, William Henry, F.G.S. *Shore, Littleborough, Lancs.*

1895, April 9. Tatton, Reginald A., M.Inst.C.E. Engineer to the Mersey and Irwell Joint Committee. 44, *Mosley Street, Manchester.*

1893, Nov. 14. Taylor, R. L., F.C.S., F.I.C. *Central School, Whitworth Street,* and 37, *Mayfield Road, Whalley Range, Manchester.*

Date of Election.

1873, April 15. Thomson, William, F.R.S.E., F.C.S., F.I.C. *Owens' Institution, Manchester.*

1896, Jan. 21. Thorburn, William, M.D., B.Sc. 2, *St. Peter's Square, Manchester.*

1896, Jan. 21. Thorp, Thomas, F.R.A.S. *Moss Bank, Whitefield, near Manchester.*

1899, Oct. 31. Thorpe, Jocelyn F., Ph.D., Demonstrator in Organic Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1899, Oct. 17. Todd, William Henry. *Greenfield, Flixton, near Manchester.*

1873, Nov. 18. Waters, Arthur William, F.L.S., F.G.S. "*Alderley.*" *McKinley Road, Bournemouth.*

1892, Nov. 15. Weiss, F. Ernest, D.Sc., F.L.S., Professor of Botany in the Victoria University of Manchester. 20, *Brunswick Road, Withington, Manchester.*

1895, April 9. Whitehead, James. *Lindfield, Fulshaw Park, Wilmslow, Cheshire.*

1901, Oct. 1. Wild, Robert B., M.D., M.Sc., M.R.C.P., Professor of Materia Medica and Therapeutics in the Victoria University of Manchester. *Broome House, Fallowfield, Manchester.*

1859, Jan. 25. Wilde, Henry, D.Sc., D.C.L., F.R.S. *The Hurst, Alderley Edge, Cheshire.*

1905, Oct. 31. Willis, Ethel G., M.A., B.Sc., Science Mistress, Manchester High School for Girls. 22, *Cawdor Road, Fallowfield, Manchester.*

1901, Nov. 26. Wilson, William, M.A. *Carron Vale*, 80, *Fitzwarren Street, Pendleton, Manchester.*

1903, Oct. 20. Wood, Harry Edwin, B.Sc. *The Physical Laboratory, The University, Manchester.*

1905, Oct. 31. Woodall, Herbert J., A.R.C.S. 32, *Market Place, Stockport.*

1902, Oct. 21. Woollcott, Walter. *Westinghouse Works, Trafford Park, Manchester.*

1860, April 17. Woolley, George Stephen. *Victoria Bridge, Manchester.*

1903, Nov. 17. Worthington, John Henry William, B.A., Assistant Master at the Manchester Grammar School. 60, *Filey Road, Fallowfield, Manchester.*

1863, Nov. 17. Worthington, Samuel Barton, M.Inst.C.E., M.I.Mech.E. *Mill Bank, Bowdon*, and 37, *Princess Street, Manchester.*

Date of Election.

1865, Feb. 21. Worthington, Thomas, F.R.I.B.A. 46, *Brown Street, Manchester.*

1895, Jan. 8. Worthington, Wm. Barton, B.Sc., M.Inst.C.E. *Kirkstyles, Duffield, near Derby.*

1897, Oct. 19. Wyatt, Charles H., M.A., *Chelford, Cheshire.*

N.B.—Of the above list the following have compounded for their subscriptions, and are therefore life members:—

Bailey, Charles, M.Sc., F.L.S.
Bradley, Nathaniel, F.C.S.
Brogden, Henry, F.G.S.
Ingleby, Joseph, M.I.Mech.E.
Johnson, William H., B.Sc.
Worthington, Wm. Barton, B.Sc.

HONORARY MEMBERS.

Date of Election.

1892, April 26. Abney, Sir W. de W., K.C.B., D.Sc., F.R.S. ***Rathmore Lodge, Bolton Gardens South, South Kensington, London, S.W.***

1892, April 26. Amagat, E. H., For. Mem. R.S., Memb. Inst. Fr. (Acad. Sci.), Examinateur à l'École Polytechnique. *Avenue d'Orléans*, 19, *Paris.*

1894, April 17. Appell, Paul, Membre de l'Institut, Professor of Theoretical Mechanics. *Faculté des Sciences, Paris.*

1892, April 26. Ascherson, Paul F. Aug., Professor of Botany. *Universität, Berlin.*

1889, April 30. Avebury, John Lubbock, Lord, D.C.L., LL.D., F.R.S. *High Elms, Down, Kent.*

1892, April 26. Baeyer, Adolf von, For. Mem. R.S., Professor of Chemistry. 1, *Arcisstrasse, Munich.*

1886, Feb. 9. Baker, Sir Benjamin, K.C.M.G., LL.D., F.R.S. 2, *Queen Square Place, Westminster, London, S.W.*

1886, Feb. 9. Baker, John Gilbert, F.R.S., F.L.S. 3, *Cumberland Road, Kew.*

1895, April 30. Beilstein, F., Ph.D., Professor of Chemistry. *8th Line, N. 17, St. Petersburg, W.O.*

1886, Feb. 9. Berthelot, Marcelin P. E., For. Mem. R.S., Membre de l'Institut, Professor of Chemistry, Secrétaire perpétuel de l'Académie des Sciences. *Paris.*

1892, April 26. Boltzmann, Ludwig, For. Mem. R.S., Professor of Physics. *Türkenstrasse 3, Vienna, IX. 1.*

1886, Feb. 9. Buchan, Alexander, M.A., LL.D., F.R.S., F.R.S.E. 42, *Heriot Row, Edinburgh.*

1888, April 17. Cannizzaro, Stanislao, For. Mem. R.S., Corr. Memb. Inst. Fr. (Acad. Sci.), Professor of Chemistry. *Reale Università, Rome.*

1889, April 30. Carruthers, William, F.R.S., F.L.S. 14, *Vermont Road, Norwood, London, S.E.*

1903, April 28. Clarke, Frank Wigglesworth, D.Sc. *United States Geological Survey, Washington, D.C., U.S.A.*

1865, Oct. 30. Clifton, Robert Bellamy, M.A., F.R.S., F.R.A.S., Professor of Natural Philosophy. 3, *Bardwell Road, Banbury Road, Oxford.*

Date of Election.

1892, April 26. Curtius, Theodor, Professor of Chemistry. *Universität, Kiel.*

1892, April 26. Darboux, Gaston, Membre de l'Institut, Professor of Geometry, Faculté des Sciences, Secrétaire perpétuel de l'Académie des Sciences. 36, *Rue Gay Lussac, Paris.*

1894, April 17. Debus, H., Ph.D., F.R.S. 4. *Schlangenweg, Cassel, Hessen, Germany.*

1888, April 17. Dewalque, Gustave, Professor of Geology. *Université, Liége.*

1900, April 24. Dewar, Sir James, M.A., LL.D., D.Sc., F.R.S., V.P.C.S., Fullerian Professor of Chemistry. *Royal Institution, Albemarle Street, London, W.*

1892, April 26. Dohrn, Dr. Anton, For. Mem. R.S. *Zoologische Station, Naples.*

1892, April 26. Dyer, Sir W. T. Thiselton, K.C.M.G., C.I.E., M.A., F.R.S., Director of the Royal Botanic Gardens. *Kew.*

1892, April 26. Edison, Thomas Alva. *Orange, N.J., U.S.A.*

1895, April 30. Elster, Julius, Ph.D. 6, *Lessingstrasse, Wolfenbüttel.*

1900, April 24. Ewing, James Alfred, M.A., F.R.S., Professor of Mechanism and Applied Mechanics. *Royal Naval College, Greenwich.*

1889, April 30. Farlow, W. G., Professor of Botany. *Harvard College, Cambridge, Mass., U.S.A.*

1900, April 24. Forsyth, Andrew Russell, M.A., Sc.D., F.R.S., Sadlerian Professor of Pure Mathematics. *Trinity College, Cambridge.*

1889, April 30. Foster, Sir Michael, K.C.B., M.P., M.A., M.D., LL.D., Sec. R.S., Professor of Physiology. *Trinity College, Cambridge.*

1892, April 26. Fürbringer, Max, Professor of Anatomy. *Grosshers. Universität, Jena.*

1900, April 24. Geikie, James, D.C.L., LL.D., F.R.S., Murchison Professor of Geology and Mineralogy. *Kilmorie, Colinton Road, Edinburgh.*

1895, April 30. Geitel, Hans. 6, *Lessingstrasse, Wolfenbüttel.*

1894, April 17. Glaisher, J. W. L., Sc.D., F.R.S., Lecturer in Mathematics. *Trinity College, Cambridge.*

1894, April 17. Gouy, A., Corr. Memb. Inst. Fr. (Acad. Sci.), Professor of Physics. *Faculté des Sciences, Lyons.*

Date of Election.

1900, April 24. Haeckel, Ernst, Ph.D., Professor of Zoology. *Zoologisches Institut, Jena.*

1894, April 17. Harcourt, A. G. Vernon, M.A., D.C.L., F.R.S., V.P.C.S. *St. Clare, Ryde, Isle of Wight.*

1894, April 17. Heaviside, Oliver, F.R.S. *Bradley View, Newton Abbot, Devon.*

1892, April 26. Hill, G. W. *West Nyack, N.Y., U.S.A.*

1888, April 17. Hittorf, Johann Wilhelm, Professor of Physics. *Polytechnicum, Münster.*

1892, April 26. Hoff, J. van't, Ph.D., For. Mem. R.S., Professor of Chemistry. 2, *Uhlandstrasse, Charlottenburg, Berlin.*

1892, April 26. Hooker, Sir Joseph Dalton, G.C.S.I., C.B., D.C.L., F.R.S., Corr. Memb. Inst. Fr. (Acad. Sci.). *The Camp, Sunningdale, Berks.*

1869, Jan. 12. Huggins, Sir William, O.M., K.C.B., LL.D., D.C.L., P.R.S., F.R.A.S., Corr. Memb. Inst. Fr. (Acad. Sci.). 90, *Upper Tulse Hill, Brixton, London, S.W.*

1851, April 29. Kelvin, William Thomson, Lord, O.M., G.C.V.O., M.A., D.C.L., LL.D., F.R.S., F.R.S.E., For. Assoc. Inst Fr. (Acad. Sci.). *Netherhall, Largs, Ayrshire.*

1892, April 26. Klein, Felix, Ph.D., For. Mem. R.S., Corr. Memb. Inst. Fr. (Acad. Sci.), Professor of Mathematics. 3, *Wilhelm Weber Strasse, Göttingen.*

1894, April 17. Königsberger, Leo, Professor of Mathematics. *Universität, Heidelberg.*

1892, April 26. Ladenburg, A., Ph.D., Professor of Chemistry. 3, *Kaiser Wilhelm Strasse, Breslau.*

1902, May 13. Larmor, Joseph, M.A., D.Sc., LL.D., Sec. R.S., F.R.A.S. *St. John's College, Cambridge.*

1892, April 26. Liebermann, C., Professor of Chemistry. 29, *Matthäi-Kirch Strasse, Berlin.*

1887, April 19. Lockyer, Sir J. Norman, K.C.B., F.R.S., Corr. Memb. Inst. Fr. (Acad. Sci.). *Science School, South Kensington, London, S.W.*

1902, May 13. Lodge, Sir Oliver Joseph, D.Sc., LL.D., F.R.S., Principal of the University of Birmingham. *The University, Birmingham.*

1900, April 24. Lorentz, Henrik Anton, Corr. Memb. Inst. Fr. (Acad. Sci.), Professor of Physics. *Hooigracht, 48, Leyden.*

Date of Election.		
1892, April 26.	Marshall, Alfred, M.A., Professor of Political Economy.	*Balliol Croft, Madingley Road, Cambridge.*
1892, April 26.	Mascart, E. E. N., For. Mem. R.S., Membre de l'Institut, Professor at the Collège de France.	176, *Rue de l'Université, Paris.*
1889, April 30.	Mendeleeff, D., Ph.D., For. Mem. R.S.	*Université, St. Petersburg.*
1901, April 23.	Metchnikoff, Élie, D.Sc., For. Mem. R.S.	*Institut Pasteur, Paris.*
1895, April 30.	Mittag-Leffler, Gosta, D.C.L. (Oxon.), For. Mem. R.S., Professor of Mathematics.	*Djursholm, Stockholm.*
1892, April 26.	Moissan, H., Membre de l'Institut, Professor of the Faculté des Sciences à la Sorbonne.	7, *Rue Vauquelin, Paris.*
1894, April 17.	Murray, Sir John, K.C.B., LL.D., D.Sc., F.R.S.	*Challenger Lodge, Wardie, Edinburgh.*
1894, April 17.	Neumayer, Professor G., For. Mem. R.S., Director of the Seewarte.	*Hohenzollern Strasse, 9, Neustadt an der Haardt, Germany.*
1887, April 19.	Newcomb, Simon, For. Mem. R.S., For. Assoc. Inst. Fr. (Acad. Sci.), Professor of Mathematics and Astronomy	1620, *P Street, Washington, D.C., U.S.A.*
1902, May 13.	Osborn, Henry Fairfield, Professor of Vertebrate Paleontology.	*Columbia College, New York, U.S.A.*
1894, April 17.	Ostwald, W., Professor of Chemistry.	*Grossbothen, Amt Sachsen.*
1899, April 25.	Palgrave, R. H. Inglis, F.R.S., F.S.S.	*Belton, Great Yarmouth.*
1892, April 26.	Perkin, Sir W. H., LL.D., Ph.D., F.R.S., V.P.C.S.	*Chestnuts, Sudbury, Harrow.*
1894, April 17.	Pfeffer, Wilhelm, For. Mem. R.S., Professor of Botany	*Botanisches Institut, Leipzig.*
1892, April 26.	Poincaré, H., For. Mem. R.S., Membre de l'Institut, Professor of Astronomy.	63, *Rue Claude Bernard, Paris.*
1892, April 26.	Quincke, G. H., For. Mem. R.S., Professor of Physics	*Universität, Heidelberg.*
1893, April 25.	Ramsay, Sir William, K.C.B., Ph.D., F.R.S., Professor of Chemistry	12, *Arundel Gardens, Notting Hill, London, W.*

Date of Election.

1886, Feb. 9. Rayleigh, John William Strutt, Lord, O.M., M.A., D.C.L. (Oxon.), LL.D. (Univ. McGill), F.R.S., F.R.A.S., Corr. Memb. Inst. Fr. (Acad. Sci.). *Terling Place, Witham, Essex.*

1900, April 24. Ridgway, Robert, Curator of the Department of Birds, U.S. National Museum. *Brookland, District of Columbia, U.S.A.*

1897, April 27. Roscoe, Sir Henry Enfield, B.A., LL.D., D.C.L., F.R.S., V.P.C.S., Corr. Memb. Inst. Fr. (Acad. Sci.). 10, *Bramham Gardens, Earl's Court, London, S.W.*

1889, April 30. Routh, Edward John, D.Sc., F.R.S. *Newnham Cottage, Queen's Road, Cambridge.*

1902, May 13. Scott, Dukinfield Henry, M.A., Ph.D., F.R.S., F.L.S., Honorary Keeper of the Jodrell Laboratory, Royal Botanic Gardens, Kew. *Old Palace, Richmond, Surrey.*

1892, April 26. Sharpe, R. Bowdler, LL.D., F.L.S., F.Z.S. *British Museum (Natural History), Cromwell Road, London, S.W.*

1892, April 26. Solms, H., Graf zu, Professor of Botany. *Universität, Strassburg.*

1869, Dec. 14. Sorby, Henry Clifton, LL.D., F.R.S., F.L.S., F.G.S. *Broomfield, Sheffield.*

1886, Feb. 9. Strasburger, Eduard, D.C.L., For. Mem. R.S., Professor of Botany. *Universität, Bonn.*

1895, April 30. Suess, Eduard, Ph.D., For. Mem. R.S., For. Assoc. Inst. Fr. (Acad. Sci.), Professor of Geology. 9, *Africanergasse, Vienna.*

1895, April 30. Thomson, Joseph John, M.A., Sc.D., F.R.S., Professor of Experimental Physics. 6, *Scrope Terrace, Cambridge.*

1894, April 17. Thorpe, T. E., C.B., Ph.D., D.Sc., LL.D., F.R.S., V.P.C.S. *Government Laboratory, Clement's Inn Passage, Strand, London, W.C.*

1894, April 17. Turner, Sir William, K.C.B., M.B., D.C.L., F.R.S., F.R.S.E., Professor of Anatomy. 6, *Eton Terrace, Edinburgh.*

1886, Feb. 9. Tylor, Edward Burnett, D.C.L. (Oxon), LL.D. (St. And. and McGill Colls.), F.R.S., Professor of Anthropology. *Museum House, Oxford.*

Date of Election.

1894, April 17. Vines, Sidney Howard, M.A., D.Sc., F.R.S., Sherardian Professor of Botany. *Headington Hill, Oxford.*

1894, April 17. Warburg, Emil, Professor of Physics. *Physikalisches Institut, Neue Wilhelmstrasse, Berlin.*

1894, April 17. Ward, H. Marshall, D.Sc., F.R.S., Professor of Botany. *Botanical Laboratory, New Museums, Cambridge.*

1894, April 17. Weismann, August, Professor of Zoology. *Universität, Freiburg i. Br.*

1886, Feb. 9. Young, Charles Augustus. *Hanover, New Hampshire, U.S.A.*

1888, April 17. Zirkel, Ferdinand, For. Mem. R.S., Professor of Mineralogy. *Thralstrasse, 33, Leipsic.*

CORRESPONDING MEMBER.

1850, April 30. Harley, Rev. Robert, Hon. M.A. (Oxón), F.R.S., F.R.A.S., Hon. Memb. R.S. Queensland. *Rosslyn, Westbourne Road, Forest Hill, London, S.E.*, and *The Athenæum Club, London, S.W.*

Awards of the Wilde Medal under the conditions of the Wilde Endowment Fund.

1896. Sir GEORGE G. STOKES, Bart., F.R.S.

1897. Sir WILLIAM HUGGINS, K.C.B., F.R.S.

1898. Sir JOSEPH DALTON HOOKER, G.C.S.I., C.B., F.R.S.

1899. Sir EDWARD FRANKLAND, K.C.B., F.R.S.

1900. Rt. Hon. LORD RAYLEIGH, F.R.S.

1901. Dr. ÉLIE METCHNIKOFF, For.Mem.R.S.

1903. Prof. FRANK W. CLARKE, D.Sc.

1905. Prof. CHARLES LAPWORTH, LL.D., F.R.S.

Awards of the Dalton Medal.

1898. EDWARD SCHUNCK, Ph.D., F.R.S.

1900. Sir HENRY E. ROSCOE, F.R.S.

1903. Prof. OSBORNE REYNOLDS, LL.D., F.R.S.

Awards of the Premium under the conditions of the Wilde Endowment Fund.

1897. PETER CAMERON.

1898. JOHN BUTTERWORTH, F.R.M.S.

1899. CHARLES H. LEES, D.Sc.

1900. Prof. A. W. FLUX, M.A.

1901. THOMAS THORP.

THE WILDE LECTURES.

1897. (July 2.) "On the Nature of the Röntgen Rays." By Sir G. G. STOKES, Bart., F.R.S. (*28 pp.*)

1898. (Mar. 29.) "On the Physical Basis of Psychical Events." By Sir MICHAEL FOSTER, K.C.B., F.R.S. (*46 pp.*)

1899. (Mar. 28.) "The newly discovered Elements; and their relation to the Kinetic Theory of Gases." By Prof. WILLIAM RAMSAY, F.R.S. (*19 pp.*)

1900. (Feb. 13.) "The Mechanical Principles of Flight." By the Rt. Hon. LORD RAYLEIGH, F.R.S. (*26 pp.*)

1901. (April 22.) "Sur la Flore du Corps Humain." By Dr. ÉLIE METCHNIKOFF, For.Mem.R.S. (*38 pp.*)

1902. (Feb. 25.) "On the Evolution of the Mental Faculties in relation to some Fundamental Principles of Motion." By Dr. HENRY WILDE, F.R.S. (*34 pp., 3 pl.*)

1903. (May 19.) "The Atomic Theory." By Professor F. W. CLARKE, D.Sc. (*32 pp.*)

1904. (Feb. 23.) "The Evolution of Matter as revealed by the Radio-active Elements." By FREDERICK SODDY, M.A. (*42 pp.*)

1905. (Feb. 28.) "The Early History of Seed-bearing Plants, as recorded in the Carboniferous Flora." By Dr. D. H. SCOTT, F.R.S. (*32 pp., 3 pl.*)

1906. (March 20.) "Total Solar Eclipses." By Professor H. H. TURNER, D.Sc., F.R.S. (*32 pp.*)

VOL. 50: PART III.

MEMOIRS AND PROCEEDINGS
OF
THE MANCHESTER
LITERARY & PHILOSOPHICAL
SOCIETY, 1905-1906.

CONTENTS.

Memoirs:

VI. The Cytological Aspect of Parthenogenesis in Insects. By C. Gordon Hewitt, B.Sc. *Plates I.—II.* - - - - pp. 1—38.
(Issued separately, May 21st, 1906).

VII. Total Solar Eclipses. (The Wilde Lecture). By Professor H. H. Turner, D.Sc., F.R.S. - - - - - - pp. 1—32.
(Issued separately, May 26th, 1906).

VIII. On the Range of Progressive Waves of Finite Amplitude in Deep Water. By R. F. Gwyther, M.A. - - - pp. 1—28.
(Issued separately, May 31st, 1906).

IX. Observations on a Captive Mole (*Talpa europæa*). By Lionel E. Adams, B.A. - - - - - - - - pp. 1—7.
(Issued separately, May 31st, 1906).

X. A New Fern from the Coal Measures: *Tubicaulis Sutcliffii* spec. nov. By Marie C. Stopes, D.Sc., Ph.D. *Plates I.—III.* - - - - - - - - - pp. 1—34.
(Issued separately, June 20th, 1906).

XI. On the Difference between Physiological and Statistical Laws of Heredity. By A. D. Darbishire, M.A. - - pp. 1—44.
(Issued separately, July 6th, 1906).

XII. Notes.—On an Allotropic Form of Arsenic and On the Estimation of Arsenic when in Minute Quantities. By William Thomson, F.R.S.E., F.I.C. - - - - pp. 1—9.
(Issued separately, August 14th, 1906).

XIII. Notes on the Palæarctic Species of Coal-Tits. By Francis Nicholson, F.Z.S. *With Coloured Plate* - - - - pp. 1—21.
(Issued separately, August 16th, 1906).

XIV. The Species of Ctenopteryx, a Genus of Dibranchiate Cephalopoda. By Dr. J. H. Ashworth and Dr. W. E. Hoyle - - - - - - - - - - pp. 1—8.
(Issued separately, August 14th, 1906).

Proceedings - - - - - - - - - - - pp. xix.—xxii.

Annual Report of the Council, with Obituary Notices of Sir J. S. Burdon-Sanderson, F.R.S., Professor Carl Gegenbaur, Dr. S. P. Langley, Mr. R. Rawson, F.R.A.S., Mr. C. J. Heywood, and Dr. George Wilson - - - - - - - - - pp. xxiii.—xxxiv.

Treasurer's Accounts - - - - - - - - - pp. xxxv.—xxxvii.

List of the Council and Members of the Society - - pp. xxxviii.—liii.

List of the Awards of the Wilde and Dalton Medals and of the Premium - - - - - - - - - p. liv.

List of the Wilde Lectures - - - - - - - p. lv.

Title Page and Contents for Volume.

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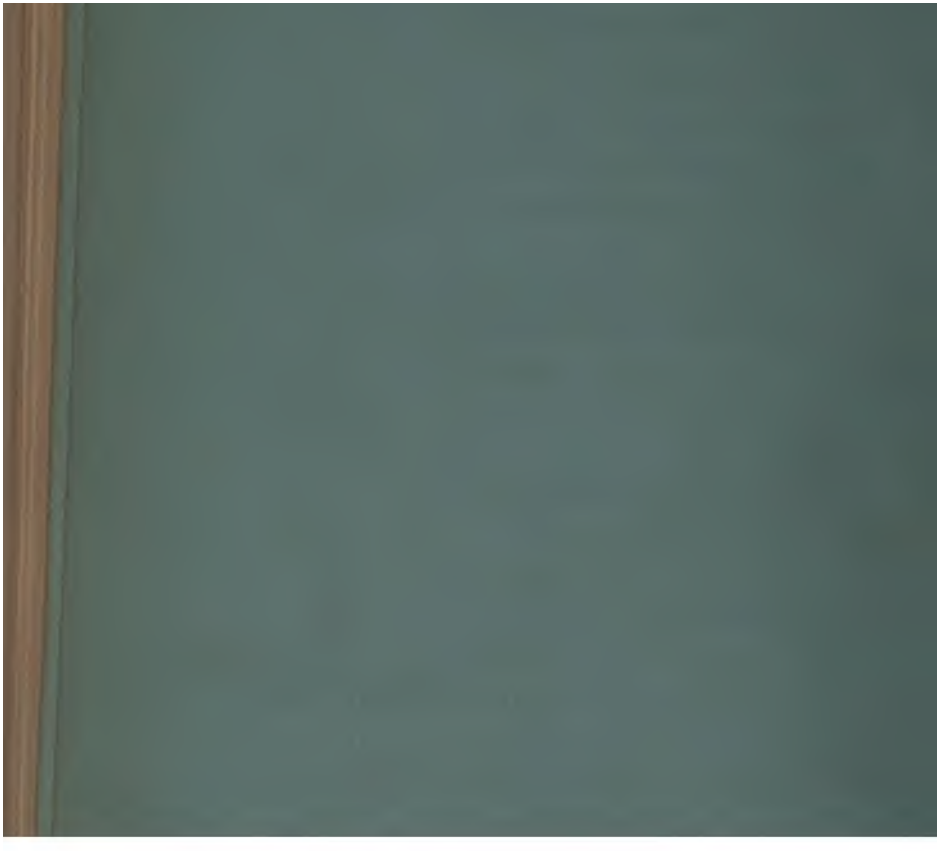
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